BRAITHWAITE'S RETROSPECT.

VOL. LXXVII. JANUARY—JUNE, 1878.

THE
RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY
W. BRAITHWAITE, M.D.
LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D.Lond.
VICE-PRESIDENT OF THE OBSTETRICAL SOCIETY OF LONDON.
LECTURER ON DISEASES OF WOMEN AND CHILDREN AT LEEDS SCHOOL OF MEDICINE.
SURGEON TO THE LEEDS HOSPITAL FOR WOMEN AND CHILDREN.

VOL. LXXVII. JANUARY—JUNE, 1878.

LONDON:
SIMPKIN, MARSHALL, AND CO.

EDINBURGH: OLIVER AND BOYD. DUBLIN: HODGES, FOSTER, AND CO AND FANNIN AND CO.

MDCCCLXXVIII.

Letters for the Editors to be addressed

DR. WM. BRAITHWAITE, Clarendon House, Clarendon Road, Leeds; *or*

DR. JAMES BRAITHWAITE, Clarendon Road, Leeds.

Parcels of Books, &c. to

Messrs. SIMPKIN, MARSHALL, & Co., London.

PRINTED FOR D. I. ROEBUCK,
AT THE OFFICE OF THE YORKSHIRE POST, ALBION STREET, LEEDS.

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SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

ANTIPYRETICS.—The question with which we have to deal at present is this: How does quinine reduce temperature? Is it by acting on the germs of disease, or by some peculiar action on the heart and bloodvessels? I have proved that quinine, salicine, bebeerine, in solution are incapable of restraining the putrefaction of fresh urine exposed to the atmosphere, but that when administered internally they are capable of reducing the temperature in febrile conditions to a most marvellous extent. It is, therefore, more than probable that quinine is transformed into some antiseptic substance in the blood, and that this body exercises a paralysing effect on the microzymes in the blood. It is now well known that a large dose of quinine is as powerful in reducing high temperature produced by exercise, as it is in cases of fever presumably due to the presence of germs in the body. It is, therefore, a matter of certainty that it produces its antipyretic effects not only by exercising its antiseptic action on the tissues, but also by its action on the bloodvessels in raising the blood-pressure,—a low blood-pressure, according to Ackermann, leading to high temperature and increased tissue-change. The actions of quinine, bebeerine, and salicine may be taken as similar, their antipyretic action being due partly to their tonic effects on the vascular system, and partly to their antiseptic action on the microzymes which are supposed to be the cause of the fever. It is well known that quinine is quite as efficacious in the treatment of rheumatic fever as salicine, but the latter, being a novelty, has attracted more attention. Salicylic acid, which is usually employed as the salicylate of soda or potash, often causes depression and vomiting when used for a few days. It occurred to me that small doses of carbonate of ammonia combined with the acid might counteract the depression and vomiting. I adopted

the following formula:—℞. Acid salicylæ, ʒ iij.; ammon. carb., ʒ iss; aquam, ad ℥ vj. Sig. A tablespoonful every three hours. On visiting the patient next day, I found that she had experienced great good from even the first dose of the medicine. (Dr. J. A. Erskine Stuart, p. 43.)

Cancer.—The amended theory of cancer recognises no cancerous cachexia except such general ill-health as gradually results from the progress of cancer. It appeals to the fact familiar to us all—familiar even in a degree which often in a particular point of view makes the experience painful to us, that the person who comes to consult us with a cancer, a person whom we may at once see to be doomed to death within a year or two, is often to all appearance in rude general health. The theory does not necessarily pretend to explain the origin of the local disease which in such a case is brought to our notice; but, starting from that as fact, it argues what must result from it. Given (it says) one primary tumour, all other facts of the case follow from it by logic of humoral sympathy; just as, in the story of syphilis, secondary and tertiary consequences need only the one hard chancre to account for them. The cancerous cachexia, like the syphilitic cachexia, is but an affair of *progressive infection;* essentially by the juices of the body—the lymph and blood, but sometimes also accidentally in other ways; an affair only of infection, of ever-widening infection, from the one first established focus of disease. How that first focus came to be, and how it came to have its wonderful endowment of infectiveness, are questions which must be separately argued; but meanwhile (says the theory) let us frankly recognise that, where our cancer-patients show certain general signs of disturbed health, presumably this "cachexia" is the effect, not the cause, of the cancer. It seems that cancers have not, as was pretended, any one structure common to them all; that on the contrary, different species of cancer have structures as dissimilar as the structures of bone and muscle. One principle of similarity does, indeed, apply to them all; not the principle of likeness *per capita*, but the principle of likeness *per stripes*. Each primary tumour has characters impressed on it, and for the most part very emphatically impressed, by what we may call its particular local parentage. The different species represent different textural origins; each texture which starts a primary cancer having, so to speak, a cancer proper to itself. Mucous and cutaneous surfaces and involutions, connective tissues, pigment-tissues, bone and periosteum, muscle-substance, lymph-gland, nerve-substance, and so forth: each has its own distinctive way or ways

of growing primary cancer; and as we study the whole range of cancerous tumours, from scirrhus to glioma, we seem to see that the growth of each makes itself only gradually divergent from the normal growth-type of the texture which it represents. And as each sort of primary cancer expresses in this way more or less clearly the organ which started it, so, of course, it is in intimate structural affinity with the non-infective tumours of the same organ; and I believe that the best histologists, when they contemplate the first textural beginnings of a cancer in any affected organ, see only such simple signs of textural overgrowth as might equally be the beginnings of a non-infective tumour. (Mr. J. Simon, p. 49.)

Drunk or Dying?—If you are called to a person with a head injury, who smells of drink, and has been undoubtedly drinking, and is more or less suffering from alcoholic poisoning, it will be the wisest plan to examine him most carefully to see if there is any injury. If there is, the question of diagnosis must be reserved. He may be relieved of his alcohol by means of the stomach-pump, &c., and then wait and see what happens. It not uncommonly happens that people are brought to the hospital in this state, and are sent away as only drunk. They may be drunk, but at the same time they may be suffering from a very serious injury. These cases are very apt indeed to happen in ordinary hospital practice, and there is not one absolute method of diagnosis. I will not go into questions as to the state of the pupils, or of an epileptiform state, which in a person suffering from drink I believe to be all futile. It is quite impossible to effect an immediate and at the same time an accurate diagnosis in these cases, and the only thing to be done is this—to try to relieve him of the alcohol by the stomach-pump, and to wait; let him lie by, in order to see what the result is. (Professor J. E. Erichson, p. 166.)

Malarial Fever.—*Quinetum.*—The name quinetum was given by my illustrious compatriot, Dr. de Vry, to the collective alkaloids obtained from Peruvian bark by a very simple process. The following conclusions may be affirmed with regard to quinetum:—1. The only malarious disease in which quinetum cannot be employed in place of quinine is pernicious fever. Quinetum requires more time to act than quinine, and as rapidity of action is absolutely necessary in this disease, quinetum cannot be used in it as a substitute for quinine. 2. In all forms of pure malarial intermittent fever, quinetum has the same apyretic effect as quinine, but is less powerful and acts more slowly. It must therefore be given in large doses and at longer intervals before the ague fit, than quinine.

3. Quinetum does not produce the unpleasant and even dangerous symptoms of quinine when given during the fit, and may be taken during the fit without causing any unpleasant feeling. 4. Quinetum never causes noises in the ear. 5. Persons who are liable to suffer from the toxic effects of quinine, and who therefore cannot take it without the greatest discomfort, can take quinetum without this unpleasant effect, and yet obtain a similar therapeutical result. 6. The influence of quinetum in chronic cases is greater than that of quinine. 7. The tonic action of quinetum is similar and perhaps even greater than that of quinine. 8. The action of quinetum in cases of masked or larval malaria, and especially in rheumatic affections due to malarious influences, is incomparably greater than that of quinine. (Dr. H. J. Vinkhuysen, p. 324.)

SEPTICÆMIA.—*Bacteria.*—Septicæmia, being the result of a virus elaborated by bacteria, a virus which does not increase in the system, is only fatal when a sufficient quantity has been introduced; whereas such a specific disease as splenic fever results from a specific organism which increases in the system, and proves fatal when it has sufficiently multiplied. Every one is now so well acquainted with splenic fever and with the already famous organism Bacillus anthracis, which produces it, that it would be out of place at any length to refer to it; but, as it is the first specific disease which we can without doubt or hesitation attribute to a specific bacterium, and must ever be alike interesting to the physician and the scientific investigator. Since proving that, when an animal is inoculated with the spores or rods of Bacillus anthracis, splenic fever is sure to result, Mr. Lister has shown that another specific organism—Bacterium lactis—always leads to a particular fermentation, the lactic acid fermentation; and, just recently, Dr. Klein has made out that the so-called pig-typhoid is produced by another specific organism similar in its growth and development to the bacillus above mentioned. (Editor of British Medical Journal, p. 32.)

SMALL-POX.—*Turpentine as an External Application.*—It at once relieves any smarting or irritation, it effectually corrects the unpleasant odour given off in the more confluent form of the disease, and seems in a marked degree to arrest pustulation, and so modify to a great extent, and, in some instances, I believe, entirely prevent pitting. Its powerful antiseptic and disinfectant properties, too, are undisputable, and in this it possesses an additional advantage in preventing the spread of the infection. I used it with great success in the epidemic of 1871-2, and since then it has given great satisfaction in

the hands of some of my professional brethren. It should be applied every night and morning by means of a feather in the proportion of one part of rectified spirits of turpentine to three or four of olive oil. I am of opinion that if this plan of antiseptic inunction were carried out in all cases, the mortality from small-pox would be considerably reduced, and its ravages proportionately checked. (Mr. Archer Farr, Lancet, May 11, p. 699.)

Typhoid Fever.—*Turpentine internally in Certain Complications of.*—My mode of giving the turpentine is as follows:—If bronchitis were present, and even if diarrhœa complicated the case, I gave what was known as my turpentine mixture. ℞. Terebinthinæ olei, ʒ ii; liquoris potassæ ʒ ii; mucilaginis acaciæ ʒ iv; syrupi papaveris albi, syrupi floris aurantii, āā ʒ viii; aquæ camphoræ q.s. ad. ℥ viii. Fiat mistura. A tablespoonful to be taken every fourth hour, the bottle being first shaken. Since I commenced that treatment, I never lost any case of typhoid from either bronchitis or diarrhœa, or from its sequelæ of ulceration or hemorrhage. (Mr. R. Perssé White, p. 46.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Diet Suitable for Brain-work.—Fat bacon is one of the most satisfactory things upon which to do hard mental work, and I invariably take it for breakfast whenever I have first to see a number of patients and afterwards to deliver a lecture. The nervous system contains a very large proportion of fat, and we can well imagine that if fat be deficient from the food, that system must necessarily suffer; and more especially is this likely to be the case if, in addition to the deficiency of fat, we have an excess of the products of nitrogenous waste such as we get from an almost exclusively animal diet. (Dr. T. Lauder Brunton, p. 333, and Practitioner, March, p. 175.)

Neuralgia.—*Salicin.*—Salicin is of use in other cases besides rheumatic fever, as in neuralgia. The cases of neuralgia in which I have found salicin of most service, are those in which the pain comes in periodic exacerbations, and in which quinine either fails to do good, or is for some reason inadmissible. It is, indeed, as an occasional substitute for quinine that salicin finds its place in the therapeutics of neuralgia. In a bad case of facial neuralgia, the patient took salicin, twenty grains at eleven, twenty at two, and forty at six o'clock, in all eighty grains. She had no pain that evening; and went to bed at ten o'clock feeling quite well. On the 10th she took the same dose, and remained free from pain. She had

no return of it; but as a precautionary measure, took twenty grains of salicin three times a day for a week. It should be given in large doses. In none of my cases was a less dose than eighty grains in twenty-four hours administered. In none did it disagree or cause any symptom more troublesome than tinnitus. (Dr. T. J. Maclagan, p. 68.)

Opium Poisoning.—*Atropine.*—A woman took from twelve to seventeen grains of opium, as nearly as could be ascertained, at 11 o'clock in the forenoon. At 2 p.m. the respiration was failing, and the pulse was weak and small. At this time the case was seen by Dr. Fothergill, who advised the subcutaneous injection of one grain of sulphate of atropia, to arrest the failure of respiration that seemed imminent. This was done at 2.15 p.m. For the next ten minutes the respiration fell till it became imperceptible. The patient was now put into a warm bed, as she was very cold from having been walked about. Ten minutes later the breathing began to return in shallow respirations, about five in the minute, with a long sigh at intervals. Improvement steadily continued till, at 4.30 p.m., the patient was breathing thirteen in the minute, the respirations being deep and long. At this time the temperature was only 97·5°, an indication of how low it had fallen. At 8.30 p.m. the respirations were 24, the temperature 100·4°, and the pulse 128, full, but compressible. The patient passed a slightly restless night; and at 10 a.m. of the 15th the respirations were 18, the pulse 100, and the temperature 99·8°. The pupils were slightly dilated. The patient was thirsty, but did not complain of much dryness of the throat. In the afternoon she was in all respects well and rational. (Dr. J. M. Fothergill, Medical Times and Gazette, Feb. 23, p. 200.)

Prolonged Syncope.—*Nitrite of Amyl.*—In a case of dangerous and prolonged syncope in a gentleman 65 years of age, nitrite of amyl was about to be administered, when the patient's breathing ceased, and it was thought he was dead. After some seconds he gave a deep gasp, and that gasp drew a good whiff of the amyl into his lungs and the breathing, such as it was, to our satisfaction, was again restored. At first I administered the amyl on a handkerchief, gradually giving larger and more liberal doses, but finding that I did not make very much progress in this way, I applied the unstoppered bottle to the nose. I thought, as the saying is, I would give the medicine "a fair trial," and a fair trial I gave it, to the saving, I believe, of the patient's life. In this way, off and on, from four o'clock in the afternoon till nearly twelve o'clock at night, in order to keep up

its good effects, I kept administering the amyl, and I believe I could not have used much less than two drachms of it from first to last. Until I gave it from the bottle freely there was no very marked appearance of flushings of the face, or much amelioration of his state, but with the flushings came gradual improvement of symptoms, and gradual heat of head, hands, feet, and body. It is now nearly three years since I first saw the great benefit which could be derived from the judicious employment of the drug, from having had occasion to administer it by inhalation to a patient suffering from intense renal dropsy with great orthopnœa, the breathing during the last few weeks of this patient's life assuming that character first described by Dr. Cheyne. This poor man derived the greatest comfort and relief from time to time from the inhalation of four or five drops of the amyl. On several occasions it seemed to restore and reinvigorate in a most extraordinary way the flickering breathing when it was apparently about to cease. (Dr. W. O'Neill, p. 95.)

Sleeplessness.—One of the most efficient means of inducing natural sleep is by the application of mustard poultices to the abdomen. In cases where sleeplessness arises from natural worry, abdominal flatus, or other annoyances, this remedy is invaluable. Schüler states that large sinapisms applied in this way produce first dilatation and subsequently contraction of the vessels of the pia-mater in trephined animals. They may thus act as do pediluvia and warm compresses to the abdomen, by diminishing the amount of blood in the brain. Preyer, of Jena, has advocated the administration of freshly made solution of lactate of soda for the production of sleep. About three drachms of carbonate of soda dissolved in warm water are neutralised by the addition of lactic acid. The solution may be given as a drink with sugar of milk or extract of beef. It usually induces sleep. He also states that the administration of a quart of fresh or sour milk, or better still of sour whey, was sufficient to induce a healthy sleep. When the sleep of a patient is broken by severe pain, opium, or its alkaloid morphia, is of value, not only by directly relieving the pain, but also by assisting the production of sleep through its influence on the cerebral circulation. Opium, according to Handfield Jones, Stillé, and others, in moderate doses produces anæmia of the cerebral vessels, with a condition closely resembling sleep; in larger and in poisonous doses, there is, according to Hammond, a venous congestion of the brain-vessels due to impaired respiratory action, as it can be removed by having recourse to artificial respiration. When the want of sleep arises from the pains of muscular spasm (as for example

that of gall-stone colic), or is accompanied by headache, flushing of the face, and other symptoms betokening a somewhat hyperæmic condition of the brain, it would seem that chloral hydrate was indicated. In the wakefulness arising from defective cardiac power, on the other hand, it frequently happens that digitalis, by strengthening the force of the heart's beats, drives the blood into the capillary system more vigorously, and relieves the congestion of the central organs and the anæmia of the extremities. (Dr. W. A. Hollis, p. 71.)

TETANUS.—First, the room must be dark and quiet; draughts are to be carefully excluded. Too much stress cannot be placed on this; the least rush of cold air, flash of light, or even sudden noise, may bring on a spasm. 2. Nourishment should be given freely, in a liquid form, and at frequent intervals; it should be always warm, cold drink being avoided for the same reason that cold air is excluded. Stimulants should be administered from the first in small quantities, say four or six ounces of brandy in the twenty-four hours, and increased if the pulse indicate it. This will hardly ever be necessary; for the pulse usually keeps up to the last, death, as I before stated, being rarely from exhaustion. 3. Never give purgatives. It is obvious that when our object is to keep the nervous system quiet we should avoid purgatives of all kinds. 4. Hydrate of chloral, together with extract of cannabis indica, is to be given in rapidly increasing doses, until the frequency and severity of the spasms are controlled. I generally commence with thirty grains of chloral in an ounce of water, and two grains of the extract of Indian hemp, in the form of pill, every three or four hours for an adult, and increase the former by fifteen grains and the latter by two grains until the desired effect is produced, when the spasms will be few and far between, the abdominal muscles almost normally flaccid, and the mouth opened to at least an inch; the patient is then in a state of stupor from which he can be roused to take nourishment. I find that sixty grains of chloral and four grains of the extract is a full dose in fairly severe cases. It seems to me that the chloral controls the frequency of the attacks, and the hemp the intensity of the muscular contractions, and consequently the dose of either drug should be increased according to these indications. It is remarkable in some cases how the disease may be controlled in this manner. Even when the patient is apparently dead from apnœa we must not give up. The moment insensibility comes on an attempt may be made to get air into the lungs by artificial respiration, and persevered with for a time. I am convinced that many deaths from tetanus may be thus averted. (Mr. A. P. Boon, p. 60.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Aneurism of the Anterior Tibial Artery.—*Esmarch's Bandage.*—In a case of aneurism of the anterior tibial artery a flannel bandage was applied from the toes to the tumour, and a second bandage from the tumour to the middle of the thigh, leaving the tumour itself exposed. Esmarch's bandage was then applied with moderate tightness from the toes to the tumour, and the patient made to stand out of bed, in order to fill the tumour well with blood. Esmarch's bandage was then applied from the tumour to the middle of the thigh, and the thick india-rubber tubing firmly fixed above it. The tumour itself being still exposed, it was noticed that the pulsation in it was quite arrested, and no bruit could be heard with the stethoscope. The patient was then directed to keep quiet in bed with his leg well raised on pillows. He did not complain of any pain till twelve o'clock (forty minutes), when he began to have the sensation of pins and needles in his foot; this pain had become so intolerable at 12.20 (one hour after the application of the bandage) that a horseshoe tourniquet was fixed firmly at the groin, and the india-rubber tubing and Esmarch's bandage removed, the flannel bandages being allowed to remain. There has never been the slightest return either of impulse or bruit: the tumour has gradually become smaller till now it cannot be felt. (Mr. Cornish, p. 169.)

Functional Affections of the Heart.—*Effect of Tea on.*—The active principle of tea—theine—is a powerful neurotic agent, and when indulged in to excess has a very decided action upon the cardiac ganglia, rendering the heart irritable, excited, and unrhythmical in its contractions. In such cases the withdrawal of the tea is absolutely essential to successful treatment. Looked at from a chemical point of view, the principles of coffee and of cocoa are closely allied to those of tea; and it seems difficult to understand how the symptoms produced by excessive indulgence in tea are relieved by substituting for it these other allied vegetable principles. Still clinically the fact remains. It is said that tea contains, in addition to its principle, theine, a volatile intoxicating oil; and it may be the presence of this agent which makes the difference. Another vegetable principle exercises a decided effect upon the heart—viz., tobacco. The effect of tobacco is to render the heart's action quicker, its beat feebler, and to promote a liability to palpitation. In the Royal Infirmary of Edinburgh this form of neurosal affection of the heart is recognised and known as "smoker's heart." In many cases this condition arises from great indulgence in strong tobacco; and very frequently the substitution of a

lighter form of tobacco in moderation is sufficient to afford relief, without the abandonment of the favourite habit. This form of nervous affection of the heart is not so common, however, as that produced by tea. (Dr. J. M. Fothergill, p. 91.)

Heart Disease.—When regurgitation is suddenly established by any accident to the valves, dilatation of the particular cavity of the heart which receives the unnatural current is produced by the additional distending cause thus arising. But I have found only a few of the cases of heart disease with sudden and fatal exacerbation to be thus explicable, and have been obliged to conclude that in the majority of cases the cause of the symptoms is some dynamical failure of the heart's muscle—probably a failure to contract at the right moment. In illustration of what may occur here, let us recall a not infrequent cause of a similar difficulty in the case of the urinary bladder. Thus, if a person allows the urine to accumulate in the bladder beyond a certain amount, he may become unable to relieve himself without surgical help, and the over-distended bladder becomes practically paralysed. It is now becoming generally recognised that, in aortic regurgitation, sudden death is frequent. When aortic regurgitation has distended the ventricle to a certain pitch, an otherwise temporary stay of a beat will become a final cessation of the beating. Any cause which induces more blood to enter the heart in the time of diastole—such a cause as great muscular exertion—would add to the effect of the missing of a beat, and in this way also exertion tends to produce sudden death in these cases. Should a similar over-distension of the left auricle occur in mitral regurgitation or obstruction, and that cavity thus become greatly widened, the result would not be so quickly fatal, for the auricle is not as essential as the ventricle to some degree of onflow of the blood, enough for life, if too little for comfort; we should thus get distress without immediate death. It is in dilatation incapable of systole that digitalis exerts its really invaluable efficacy. I venture to say that whenever the heart's cavities are thus dilated, digitalis is the remedy indicated; and this is true whichever may be the valve whose disease may have caused the dilatation. Some writers assume that in aortic disease digitalis is injurious; but, in fact, when dilatation that cannot be closed is reached, *and produces its proper symptoms*, in aortic disease, digitalis is the proper remedy there also. I have found that where digitalis has exerted its benefit to the full, and no more good can be got from it whilst the heart is still giving way, a further check to the symptoms can be obtained by giving tincture of belladonna along with it, in the proportion of ten minims of

tincture of belladonna to fifteen of tincture of digitalis. In cases of urgent danger from dilatation with mitral disease, I have given as much as one drachm of tincture of digitalis, in one dose, with marked relief; at other times half a drachm, repeated in two hours. For the sake of prognosis, it is very important to be able to measure the extent of regurgitation in valvular disease. The pulse does this best. For instance, a full splashing pulse in aortic disease, or a small weak one in mitral disease, is of bad omen, especially the former. But when, in aortic disease, the pulse is small or of moderate volume, the case may do well, in spite of two-and-fro murmurs. Thus, I saw an old gentleman three years ago whom Dr. Golding Bird had, thirty years previously, warned against running, "because the valves of the great artery of the heart were diseased." He had loud to-and-fro murmurs, but a small quiet pulse, and he died at last of a carcinomatous disorder of the spine. Several such cases I could relate. One man came to me as a life office case, having had a single attack of rheumatic fever fifteen years ago, and no rheumatism at all since that time; he had a loud to-and-fro murmur of aortic disease, but a small steady pulse, and was in good health. Also in mitral regurgitation, as Dr. Barlow used always to insist, the smallness of the pulse as a measure of the regurgitation is a far more important sign than any the stethoscope can give. (Dr. W. Moxon, p. 75.)

MITRAL STENOSIS.—*Etiology.*—Mitral stenosis owns a *rheumatic* origin in the majority of instances, at least in 60 per cent., the term being employed in a comprehensive, but not inexact, sense, and it would probably not be wrong to believe that two-thirds of all the cases are the result of rheumatic disease. In the present state of knowledge upon the subject, no other etiological cause can be so distinctly connected with this lesion. There appears reason to believe that the mitral valve is sometimes affected in children in the course of the exanthemata, or may be prone in young persons to slow degenerative changes resulting in stenosis (Fagge). (Dr. Dyce Duckworth, St. Bartholomew's Reports, 1877, p. 270.)

NÆVUS.—The author describes his treatment of nævus as follows. Assisted by Dr. Crompton, I passed three acupressure pins horizontally, at equal distances, and parallel to each other. After puncturing the skin with the point of the pin, I passed it on in a rotatory kind of way, something like darning a stocking, backwards and forwards, taking care to keep the point away from the skin and mucous membrane, until it emerged at the opposite side of the lip. Endeavouring in this way to secure compression of all the veins, and at

the same time to include them all in the grasp of the pin. This I believe would have been sufficient, but in order to make quite certain, I passed a ligature of worsted, in the figure of eight fashion, over the pin, but not sufficiently tight to impede the circulation in the skin. The worsted was allowed to remain three or four days; but the pins were not removed for a fortnight, the parts gradually shrunk, the veins became obliterated, the darkened purplish skin resumed its healthy hue without any cicatricial marks—and in about eight weeks the lip had so far resumed its natural size and shape that it would have been difficult to trace any sign of the nævus. No convulsions, local inflammation, or symptoms of constitutional irritation at any time supervened. (Mr. Andrew Bontflower, p. 334.)

Spontaneous Arrest of Bleeding from Divided Arteries. —The retraction of the ends of a divided artery within its sheath depends on the action of the longitudinal elastic fibres which form the inner layer of the outer coat of the vessel, while the contraction of the mouth of the artery is due to the action of the circular muscular fibres composing the middle coat. In consequence, however, of the proportionally less developed muscularity of the middle coat of large arteries than of that of small, the contraction is correspondingly less in large arteries. Hence the formation of an external clot plays a more important part in the suppression of hemorrhage from large arteries when cut and left to themselves. This difference between the larger and smaller arteries, in addition to their actual difference of width, is the cause why in the larger vessels a clot of a size sufficient to stop up the gaping mouth of the vessel takes a longer time to be formed, and why, consequently, the bleeding may not stop of itself before it has gone on to a dangerous extent. (Mr. T. Wharton Jones, p. 172.)

Tendon Ligatures for Arteries.—The tendons of the stag, ox, or horse are available for procuring these ligatures; especial care, of course, being taken in the case of the last animal as to the cause of death. The best tendon for splitting easily and regularly is the flexor perforatus in its course below the heel (os calcis), giving, in the horse, ligatures more than a foot in length. They will be somewhat shorter if the ox-tendon be used; but, I think, finer ligatures may be obtained from this animal, the tendons of which are easily procured. A hundred ligatures may be got from one tendon. The perforans tendon is long, and might be supposed well adapted for the purpose; but it is more compound in its formation, and, therefore, difficult to split. The tendon, before splitting, should be cleared of the surrounding cellular

tissue, and the inner or front surface of the perforatus is best stripped off, as it is a cause of resistance in the splitting and of irregularity in the ligatures. When split to the required thickness, the ligatures may be sorted and kept in proof spirit containing some carbolic acid. They are applied best in this moist state, simply rendered a little drier by wiping; or they may be used dry, which they soon become when left exposed; they may also be twisted a little and stretched by pinning down at the ends. When they are used somewhat moist, but not too much so, as they are then slippery to handle, their advantages are: first, great strength; secondly, that a vessel may be tied with them in a knot as easy to form, and as little likely to slip, as with hemp or silk, the tendons being equally supple; and, thirdly, that they will, when cut close, certainly in a short time dissolve and disappear. I have found that a medium sized ligature, placed on a suppurating surface, dissolves in seven or eight days; probably the knot requires a longer time. This may be thought too rapid a solution, but they have been applied to my knowledge in half a score of cases, and two or three times to the main arteries, and have never been followed by hemorrhage or other bad effects. (Mr. R. Garner, Brit. Med. Journal, Dec. 8, p. 803.)

VARIX.—*Subcutaneous Injection of Alcohol.*—By means of an ordinary hypodermic syringe, from fifteen to twenty drops of a mixture of alcohol and water, in equal parts, are injected into the cellular tissue beneath the vein, which, together with a fold of skin, has been previously raised by the thumb and forefinger. The injection gives rise to a small swelling, and on close observation the vein may be seen to contract. More or less infiltration is observed on the third day, and in very sensitive patients the skin is apt to become red, and even a small abscess may form, the vein itself not becoming involved in the suppuration. As the infiltration becomes firmer and smaller the vein also diminishes in size, and gradually becomes hard and cord-like. In some cases one such injection many suffice to effect a cure of the varix, but in the majority the operation has to be repeated several times. The results are most successful when the dilated veins form a plexus, but the treatment is more difficult when there are many branches. The pain during and after the operation is very slight; the length of time required for the subsequent treatment varies according to the gravity of the case. In cases where the result is not entirely successful the operation appears to be a valuable auxiliary to other palliative measures. The author claims for his method that it is absolutely free from danger. He was induced to make trial of it for the cure of varix in

consequence of the excellent results he obtained from the use of similar injections for the radical cure of hernia. (Dr. Englisch, of Vienna, Medical Examiner, No. 112.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

Bronchitis.—*Eucalyptus Globulus.*—The Eucalyptus Globulus, or Blue Gum Tree of Tasmania, has remarkable anti-catarrhal virtues. The only preparation which I have used has been the tincture prepared by several of our most eminent druggists in Edinburgh, and I have seldom prescribed more than a teaspoonful, mixed with a wineglassful of water, twice a day. In several cases of bronchitis with profuse expectoration, I have witnessed remarkable benefit after a very brief use of the remedy, evinced by a rapid diminution of the discharge, and also by a corresponding improvement in the general condition of the patient. (Mr. Benj. Bell, p. 329.)

Nitre-paper Fumigation.—In the aged and enfeebled bronchitis may be benefited by the nitre-paper fumigation. We are only too familiar with cases of senile bronchitis in which prolonged fits of violent and exhausting cough are ineffectual to the clearance of the bronchial tubes and trachea. In any such case it will be well to try the effect of nitre-paper fumigation. It is found that the inhalation of the products of its combustion excites usually effectual cough, acts as a very efficient expectorant, and allays spasm. Some years ago I recommended to Messrs. J. Bell and Co. that they should paint over this nitre-paper several coatings of Friar's Balsam, and I have found this to be a very useful addition. The paper ought to be the coarsest brown paper procurable, the old cordage in it proving of value in giving off tarry empyreumatic fumes, and these together with the oxygen gas evolved from the potassic nitrate, and the benzoic acid from the compound tincture of benzoin, form a suitable stimulant and balsamic smoke for inhalation. The bed-curtains should be drawn around the patient while the paper is burnt, so that a fully charged atmosphere may be inhaled, and a piece of paper not less than four or five inches square should be used each time. The process may be repeated several times a day. (Dr. Dyce Duckworth, p. 331.)

Cod Liver Oil in Bronchitis.—In bronchitis, both acute and chronic, a little cod-liver oil is generally much more serviceable than cough mixtures, and patients express themselves very grateful for the relief which it affords by lessening the cough. Indeed in many cases of chronic bronchitis, it seems to me to

be almost the only remedy which affords any marked relief. (Dr. T. Lauder Brunton, p. 333.)

Cold in the Head (Coryza).—*Salicin.*—I had a bad cold in the head and felt ill and miserable. I determined to try the effect of salicin, and took twenty grains every two hours. I took the first powder at eight in the morning, and continued to take it every two hours during the day. After the third dose I was quite sensible of feeling better; and when evening came, having taken in all 160 grains, my cold was gone—I felt quite well. I took forty grains at bed time, and got up the next morning perfectly well. As a precautionary measure, I took the salicin in twenty-grain doses four times a day for a couple of days. I did not use any other means to get rid of the cold, and did not even remain in the house. I have frequently had such colds, but never got rid of one so quickly before. My little boy, aged six, had a bad coryza, with all the usual symptoms. He rose with it on him on the morning of the 6th May, 1876. I gave him eight grains of salicin every two hours; and by evening when I saw him again, having had six powders (forty-eight grains), his cold was nearly gone. The next morning he was quite well. (Dr. T. J. Maclagan, p. 97.)

Nasal Polypus.—There are several varieties of nasal polypus. The principle of treatment is the same in all: to remove as cleanly as possible the whole of the growth, to detach thoroughly the base of its stalk—to destroy its roots, as some say. In order to secure its thorough extirpation, we do not think of attacking the bulk of the tumour; its neck and base are the parts at which we aim. We may grasp the pedicle with strong forceps, or secure it by a ligature, or use the knife or *écraseur*, or cautery, or carry out the principle in other ways: but the operation is never satisfactory when the polypus is brought away piecemeal or torn and mangled. The simple object is, I repeat, to bring the whole thing away at once by its neck, and to leave a bare surface, with no fragment projecting beyond the level of the surrounding mucous membrane. In the case of the more doubtful forms, it is well to cauterise the surface afterwards. (Mr. W. S. Savory, p. 174.)

Naso-Pharyngeal Disease.—*Iodoform as a Local Application.*—Iodoform may be applied in two ways: first, as an ethereal solution (one part of iodoform to ten or twelve of common ether), with a brush or on a piece of sponge or cotton wool in a suitable holder. The application should be made first to the throat, passing the brush up behind the uvula, and

then very thoroughly by another instrument up each nostril. This procedure may, if the mucus be inspissated, be preceded by use of the posterior nasal douche. The only pain is that due to the discomfort of introduction of a foreign body into sensitive passages and to the evaporation of the ether. In any case, the smarting is but momentary. After the application, which should be repeated twice or thrice a week, the mucous membrane will be noticed to be much paler in colour, and to be covered by a thin film of the iodoform. After about six repetitions, the swelling will be found to have much decreased. The second method of using iodoform to these parts is one which may be pursued by the patient; namely, mixed with vaseline and applied with a small brush far up each nostril. This can be done night and morning, or on the alternate days of the stronger application. The form that I employ is five to eight grains of iodoform (with sufficient ether to dissolve it) to one ounce of vaseline. (Mr. Lennox Browne, British Medical Journal, Feb. 9, p. 193.)

Iodoformed Wool.—It is more especially in diseases of the nose and postnasal region that the proved curative influence of iodoform requires to be more extensively known. In cases of rhinitis, ozæna, postnasal catarrh, and hyperplastic deposits, whether simple or syphilitic, iodoform exercises quite a specific influence. As regards the best method of using it topically in these sensitive regions, the objection to the ethereal solution is its extreme painfulness. This defect—a very serious one in the case of delicate females—is due entirely to the solvent employed, iodoform itself having a distinctly anodyne influence on the tissues to which it is applied. Allowing the ether to evaporate somewhat from the sponge or brush employed modifies this unpleasant effect: but even then I have observed patients shrink from its application with expressions of the greatest dismay. I have, therefore, sought for some vehicle for iodoform, which, while free from the objections due to the ether, will enable the drug to be maintained in contact with the tissues to be influenced by it. Finely carded cotton-wool appears to supply this requirement; an "iodoformed wool" has been prepared for me by Messrs. Bullock and Co., which has yielded very satisfactory results in practice. Each drachm of the wool contains a drachm of iodoform, with which it is very intimately blended. For use, it is simply necessary to pass on a probe a small portion of the wool to that part of the nasal cavity which may be deemed necessary. Here it will remain for a period varying from one to twenty-four hours, its presence being unrecognised by the patient. (Dr. E. Woakes, British Medical Journal, Feb. 9, p. 193.)

PHTHISIS.—*Koumiss.*—In phthisis, in the early stages, where capriciousness of appetite or disinclination for any food is a prominent symptom, as also in the laryngeal form of the disease, we possess in koumiss an agreeable and efficacious form of nourishment. In cancer of the stomach or rectum, in the vomiting of pregnancy, in mesenteric disease, in affections of the throat accompanied by dysphagia, in gastric catarrh or gastric ulceration, koumiss will be found valuable as a remedy and as food. Some patients may consider that it is a somewhat expensive medicine; but, if it be explained that it is also food, and that it is certainly cheaper than wine, this objection is readily overcome. (Dr. Llewelyn Thomas, British Medical Journal, Feb. 9, p. 193.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

COD LIVER OIL.—*Administration of.*—The best time to give cod-liver oil is an hour or so after, instead of immediately after, a meal, as it will then have a shorter time to stay in the stomach, and will pass out quickly into the duodenum. The oil is better given in the form of an emulsion with gum acacia, than with solution of potash or carbonate of potash, because the gum is little, if at all, affected by the gastric juice, whereas the potash will be neutralised, and its emulsifying properties destroyed, so that the particles of oil can again run together. (Dr. T. Lauder Brunton, p. 173.)

DISEASE OF THE STOMACH.—*Eucalyptus Globulus.*—A gentleman of seventy-five had suffered from formidable disease of the stomach for eight or ten years, and on several occasions had seemed very near his end, with every symptom of malignant ulceration. Great quantities of blood had been vomited from time to time, and at short intervals, seldom exceeding a fortnight, the stomach after becoming painfully distended with a sour *barmy* fluid, was relieved by repeated vomiting, while life itself seemed possible only with extreme lightness of diet and most vigorous self-denial. A strong, active man had become a confirmed invalid, and seemed both to himself and others beyond the reach of remedies. He has taken the tincture of *eucalyptus* twice daily for many months, and during all that time has scarcely had even a threatening of those painful and exhausting attacks which had latterly occurred almost every week. The only preparation which I have used has been the tincture prepared by several of our most eminent druggists in Edinburgh, and I have seldom prescribed more than a teaspoonful, mixed with a wine-glassful of water, twice a day. (Mr. Benjamin Bell, p. 329.)

INFANTILE DIARRHŒA.—*Oxide of Zinc.*—Oxide of zinc is described as tonic, sedative, and astringent, and when it was considered that it had enjoyed a high reputation in nervous complaints, such as spasmodic asthma, chorea, certain forms of convulsions, and had been employed with success in chronic dysentery, as also in chronic bronchitis, we were hopeful that in infantile nervous diarrhœa, or, as we are more inclined to call it, intestinal chorea, it might prove serviceable. The plan adopted at first was to give to a child of six months two grains of the powder every six hours, and generally after the third dose a distinct improvement was observed. As it was inconvenient in the form of powders, more especially for hospital patients, it was made up as a mixture with mucilage and water. Cases which had gone the length of the dysenteric type yielded to this treatment, and as case after case was benefited by it we saw that in oxide of zinc we had a most valuable agent for the treatment of this form of infantile diarrhœa. (Dr. J. C. Renton, p. 103.)

FAT AND ITS USES.—Fatty, starchy, saccharine, and albuminous articles are all capable of being converted into fat. The fats themselves are partially emulsified, as well as partially saponified by the pancreas, and, passing through the lacteals, mesenteric glands, and thoracic duct into the general blood-current, increase the amount of fat in the blood. Other articles of food will increase the proportion of fat, when not taken in excess. The late Dr. Hughes Bennett used to say: "The main causes of tuberculosis were the dearness of butter and the abundance of pastry-cooks, the poor not getting sufficient fat, and the upper classes disordering their digestion by puff-paste." The best time to give cod-liver oil is an hour or so after, instead of immediately after, a meal, as it will then have a shorter time to stay in the stomach, and will pass out quickly into the duodenum. The oil is better given in the form of an emulsion with gum acacia, than with solution of potash or carbonate of potash, because the gum is little, if at all, affected by the gastric juice, whereas the potash will be neutralised, and its emulsifying properties destroyed, so that the particles of oil can again run together. The nervous system, containing a great proportion of fat, must necessarily suffer if fat be deficient in the food. He believed that the glycosuria of gouty subjects was due to the same cause as the excessive production of urates—viz., deficient oxidation; for the sugar and the excess of uric acid may alternate in the urine, or both may be wanting, and then fat is

deposited. Seegen has noticed that diabetes is often preceded or accompanied by an immense accumulation of fat; and the gouty glycosuria of middle age often occurs in very stout persons. (Dr. T. Lauder Brunton, p. 332.)

HEMORRHOIDS AND PROLAPSUS OF THE RECTUM.—*The Clamp and Cautery.*—The author has operated with invariable success in 530 cases, and still prefers his old plan. He says: I am glad to find that the ligature is gradually giving way to the clamp and cautery, and I doubt not it will in time be entirely abolished. As I have before stated, I have made no alteration in the mechanism of the instruments. I am old-fashioned enough to use the iron still heated in the fire, and not by the ingenious lamp constructed for me by Mr. Matthews, in using which, on one occasion—through the imperfection of the valve—the lighted spirit escaped, and nearly set fire to the patient's house. Nor do I use the thermo-cautery. I still make use of the clamps furnished with ivory wings, which my old friend, Dr. Wiblin, has ridiculed by terming them a "pretty toy." I find that the Messrs. Matthews construct these clamps with the utmost nicety, and I have had no mishap with them. I am glad, also, to be able to state that in not one single instance have I failed to arrest hemorrhage by the cautery. (Mr. Henry Smith, Lancet, April 20, p. 562.)

INTERNAL HEMORRHOIDS.—*Glycerine.*—In a case of piles, after everything else had been tried, the bowels regulated as far as possible by diet, frequent ablution with cold water carried out, and various astringent applications used, but all to no purpose, I resolved to try glycerine, and ordered two teaspoonfuls to be taken in a little water morning and evening. The result has been most satisfactory. She has now taken it for more than a year, during which time she has not had the slightest trouble from the pile. Several other cases were equally successful. No explanation can be given of this use of glycerine. It was discovered accidentally during its administration as a drink—diluted of course with water—in a case of diabetes. (Dr. David Young, of Florence, p. 180.)

DISEASES OF THE URINARY SYSTEM.

ALBUMINURIA.—*Albuminous Substances which occur in the Urine.* Albumins and globulins both combine with mineral acids and alkalis to form acid-albumins and alkali-albumins, or alkali-albuminates, as they are generally called. Thus, if we take a little white of egg dissolved in a quantity of water, we get a solution which is coagulated by boiling. But if we first

add to it some very dilute nitric or hydrochloric acid (*e.g.*, its own bulk of four parts commercial acid in 1000 of water), and then heat it, we may boil it as much as we please, but no coagulum will form. The albumin has combined with the acid and formed acid albumin, which is not coagulated by heat. This is the reason why carelessness in washing out test-tubes sometimes causes the presence of albumin in urine to be overlooked. Let us suppose that a man tests urine in the usual way, either by boiling and adding nitric acid afterwards, or by nitric acid alone, and afterwards throws out the mixture of urine and acid. He then pours some fresh urine into the tube without washing it and proceeds to boil. The urine remains clear, and he supposes it to be free from albumin, yet it may be highly albuminous. For the acid diluted by the urine first tested clings to the side of the tube, and and being thus heated with the second urine, gradually converts it into acid-albumin; and by the time the coagulating point of the unchanged albumin is reached, there is no longer any to coagulate, the whole having been changed into acid-albumin. By boiling a solution of white of egg or some albuminous urine with liquor potassæ, instead of dilute acid, the albumin in either solution will be converted into alkali-albumin, or alkali-albuminate, and will not be coagulated by boiling. By its conversion into acid-albumin or alkali-albumin, ordinary albumin undergoes another change besides the loss of its coagulability on boiling, for it loses also its solubility in water. White of egg, or the dried albumin from serum or urine, may be dissolved in water and give a neutral solution, but after it has been changed into acid-albumin or alkali-albumin it becomes insoluble in water, and is therefore precipitated from its acid or alkaline solutions by neutralising them. When the point of neutralisation is passed, and the solutions rendered alkaline by alkalis or their carbonates, or acid by mineral acids, the precipitate is redissolved, but will again be thrown down by neutralising. But if acetic acid be used instead of a mineral acid to neutralise a solution of alkali-albuminate, the precipitate is not dissolved by a slight excess of acid. Indeed, when sodium phosphate is present in the solution, alkali-albumin is not precipitated by exact neutralisation, and the precipitate only falls after the liquid has been rendered acid. When we wish, therefore, to separate alkali-albumin from a liquid, we acidulate with acetic acid. By then boiling we can precipitate both the ordinary albumin and the alkali-albumin from a fluid which contains them, while if we boiled without previously adding acetic acid, the ordinary albumin only would be coagulated, and on removing it by filtration the fluid would be found to contain alkali-

albuminate. But besides its use in precipitating alkali-albuminate, acetic acid possesses the power of causing ordinary albumin to coagulate more readily on the application of heat, and its addition to any fluid from which we wish to separate albumin thus serves a double purpose. (Dr. T. L. Brunton and Mr. D'Arcy Power, p. 105.)

BLADDER AFFECTIONS.—*The Local Treatment of.*—I have recently been using in the local treatment of affections of the bladder soluble pessaries, introduced by means of a special instrument manufactured for me by Messrs. Krohne and Sesemann, and which I have designated a pessary-catheter. The instrument consists of a metallic catheter, open at the end, into which is received a cocoa-nut butter pessary, containing the requisite drug. After the urine has been allowed to run off, by pressing the stylet, the pessary is projected into the bladder, when the instrument is at once removed. The pessaries contain various agencies, including morphia, opium, bismuth, nitrate of silver, perchloride of iron and belladonna. They are so shaped as to form an end for the catheter; and their exposed surface is hardened by a layer of spermaceti, so as to prevent their becoming dissolved in their passage down the urethra. (Mr. Reginald Harrison, p. 218.)

CYSTITIS WITH OFFENSIVE URINE.—*Quinine Solution as a Local Application.*—In a case of necrosis of the tibia, a long trough was left after removal of the sequestrum in which pus accumulated and became fetid. A quinine solution was used as a bactericide. The result from the local exhibition of the quinine was most satisfactory. We used a solution of the disulphate of quinine, one grain to the ounce of water, the smallest quantity only of sulphuric acid that would suffice to complete the solution of the alkaloid being added. I have since made frequent use of the solution of quinine as a local application. It appears to me to be especially efficacious, either alone or combined with the bichloride of mercury or the chloride of zinc, in certain forms of soft chancre. The most striking result, however, is obtained by injecting the solution of quinine into the bladder in those cases where the urine is loaded with pus, and is *intensely offensive;* the bladder being irritable, the desire to urinate recurring every hour, or more often, for example, where the bladder only imperfectly empties itself, or when the continual use of the catheter is called for in enlarged prostate, or in atony of the organ. Within the past few days I have been informed by a patient who has habitually had recourse to the catheter, the urine voided being alkaline and highly offensive, that the injection of the

quinine solution has been followed by such an abatement of the sensitiveness of the neck of the bladder that the desire to micturate comes on now only after the lapse of six or seven hours, in place of after the lapse of every hour or hour and a half. The following is the method of using the quinine as a bladder injection:—Dissolve twenty grains of disulphate of quinine in twenty-five ounces of water by the aid of a few drops of dilute sulphuric acid or a teaspoonful of *common brown* vinegar. Of this solution inject into the bladder two or three ounces and let it remain. (Mr. T. W. Nunn, p. 217.)

Diabetes Insipidus.—A medical man retired from active practice at forty-five years of age, had for many years been the subject of diabetes insipidus. He was ordered one drachm of dilute nitric acid in a quart of water, to be used daily. I confess I was much and agreeably surprised to find that literally in the course of eleven days, this gentleman was cured, and though he had subsequently other disease, the morbid thirst never returned. That it is a most potent medicine admits of no doubt; and that its curative effects are, often at least, only seen when it is given in very full doses is equally certain. Had I not acted on this idea, it is certain I would have failed in some of the cases which have been detailed. It will be recollected that in three of these the doses respectively were two, four, and five drachms in the day. The power of raising a kind of fever in the system I take to be of great value in therapeutics. Nor is it confined to the medicines named. Iron will cause it, and if I mistake not, so will arsenic. But probably few will cause it as rapidly and safely as the acid I have brought under notice. (Dr. Henry Kennedy, p. 118.)

Gouty Glycosuria.—In persons of a gouty temperament, living to a great extent on animal food, especially when they reach middle age, we not unfrequently observe sugar in the urine. It is, I think, a mistake to term this diabetes; it should rather be called gouty glycosuria. The cause of it appears to be that the oxidation in the body is insufficient to consume all the substances taken in as food, and one or other of them must needs undergo imperfect combustion. Accordingly we find that it is sometimes the nitrogenous products of waste which pass out in a state of imperfect oxidation, large quantities of uric acid and urates appearing in the urine instead of urea. At other times it is the non-nitrogenous products, such as sugar and fat, which escape oxidation, the uric acid being absent from the urine whilst sugar appears, or both uric acid and sugar may be wanting, and fat is

accumulated. These processes of imperfect oxidation seem to be very closely connected. (Dr. T. Lauder Brunton, p. 335.)

Phosphatic Deposits in the Bladder.—Among elderly men who are compelled to pass much of their urine, or all of it, by catheter, and occasionally when this condition does not exist, the bladder often becomes the seat of phosphatic precipitates. These may be passed as greyish-white granular particles, or in little masses, or they may be retained and form "concretions" of the size of a pea or larger, in any case producing more or less painful and frequent micturition. The issue of such a case, if unrelieved, is never doubtful; a phosphatic calculus is certain to be formed, and, if not removed, becomes large. Indeed, no calculous product is deposited so rapidly as this; consequently there is none which attains so great a size if its progress be not checked. These deposits should be removed when first formed by washing out the bladder, or, if this is not successful, by the flat-bladed lithotrite. Nothing is easier, more simple, or more safe in execution than the accomplishment of this in their early stage; the prevention or cure of the diseased action which produces them is another subject, to be presently considered. At any rate, they should be mechanically removed before they have any claim to be considered as "stone," which they will certainly become if they are let alone. In every case in which such removal is accomplished we have the satisfaction of knowing that the production of a stone has been avoided. And were the lithotrite capable of doing nothing more than rid the bladder of these deposits, it would still hold a high rank among the resources of surgical art. (Sir Henry Thompson, p. 203.)

Cysto-Phosphatic Deposits.—Every surgeon of experience knows that a man of advanced age may not once have emptied his bladder during several years, and that he may nevertheless lead an active life, subject to no other annoyance than that of frequent calls to micturate; the capacity of the reservoir being impaired exactly in proportion to the quantity of fluid abnormally retained therein. Notwithstanding this defect, the urine passed by this man may be perfectly clear, showing no sign of decomposition or trace of precipitated phosphates. The one thing I have referred to has been absent—viz., inflammation of the mucous lining of the containing organ. Let this once occur—and it may arise spontaneously, or it may be produced by the first catheterism, however carefully done (the mechanical disturbance of the bladder sometimes suffices in the condition described to set up slight inflammation)—and decomposition of the urine

begins to take place, and triple phosphates may soon be observed. The following are the conclusions I have arrived at in relation to this subject:—1. That, in its healthy condition, the bladder rarely, if ever, retains, but on the contrary expels, all phosphatic deposits. 2. That when the bladder is not healthy, but affected by chronic inflammation, provided it is not considerable nor very prolonged in duration, the power of expulsion is still almost as great as in the healthy organ. 3. That there is a diseased condition of the inner coat of the bladder, in which its ability to expel phosphatic deposit is almost lost, and in which the formation of concretions—and if these are neglected, of stone—is certain to occur. It by no means infrequently happens after cystitis that the mucous coat acquires a morbid condition, which is not so much one of actual inflammation as the result of long continuance of that action. The membrane loses its polish, usually in one or more circumscribed spots, and becomes abraded, roughened, even flocculent, an exudation of lymph sometimes takes place on the surface. This matter, which is extremely tenacious, and to which phosphatic salts strongly adhere, is wholly different, it need hardly be said, from the ordinary and well-known viscid mucus of the bladder. The latter has often been regarded, not without apparent reason, as a mechanical agent for gluing together crystalline particles to form concretions, although I doubt that it acts thus to any considerable extent. The lymph exuded from an abraded spot becomes loaded with phosphates, attaching them to the surface beneath, from which the tenacious mixture is not easily removed. (Sir Henry Thompson, p. 206.)

Treatment of Phosphatic Deposits and Concretions.—What is to be done when phosphatic deposits and concretions are formed in the bladder and show a tendency to remain, or, after expulsion to be again produced? The first condition indisputably necessary to success is that the organ, if incapable of emptying itself, should be artificially emptied by the patient in the easiest manner, as often in the twenty-four hours as his comfort demands, and never less than twice a day, however small the quantity left behind. Next, as organs thus affected are by no means always quite emptied, even by the catheter, a small quantity of warm water should be injected once, twice, or thrice daily after catheterism, to wash out the remaining urine if any such there be, and the phosphatic precipitate which will be certainly found therein. For this purpose, the four-ounce india-rubber bottle with brass nozzle and stopcock is the best instrument; one-third only of its contents is to be injected at a time, and this quantity

is to run out before the succeeding third is introduced. To the water should be always added either carbolic acid in the proportion of one grain to the ounce, or the solution of permanganate of potash (Condy's) six or eight minims to the ounce. Either of these disinfectant solutions, the first-named being perhaps mostly preferable, should be employed as preliminary to all other injections; they are not in the slightest degree irritant to the bladder, and they deodorise and cleanse the interior. Further, and this is a fact of some importance, carbolic acid does not decompose any solution of metallic salts which it may be desirable to inject immediately afterwards. It ought not to be necessary to add, in passing, that all instruments should be placed, before and after use, in a bath of carbolic acid solution, but double the strength of that mentioned above. This, of course, relates to all instruments which are at any time or for any purpose to be introduced into the urinary passages. The bladder being thus kept in good sanitary condition, the next consideration is, what agents are to be employed to promote healing action in the diseased mucous membrane? The best are salts of silver, copper, and lead, very weak solutions of which should be used at the first occasion of applying them, watching carefully the result before augmenting their strength, and doing so very gradually. The nitrate of silver should at first not exceed in strength the proportion of one grain to four ounces of distilled water; even one to six ounces is preferable if a patient is more than usually susceptible. It should always be preceded by a cleansing or deodorising injection, to remove from the surface to be acted upon the muco-pus which is coagulated by the solution of silver, and tends to hinder contact with the agent. This injection is to be employed in the gentle manner directed above for the first application. If very little inconvenience follows, a slightly stronger solution should be used after an interval of two or three days, always avoiding an increase in strength sufficient to produce any severe or long-continued pain. Sulphate of copper should be applied in the same proportion—viz., one grain to six or four ounces of distilled water. An acetate of lead solution of the same strength is a valuable agent, to be used daily, or even twice a day, by the patient himself; but the sulphate of copper, like the nitrate of silver, is to be repeated only every alternate or third day, according to results. (Sir Henry Thompson, p. 209.)

Stricture of Urethra.—*Prevention of Organic Stricture.*— We must prevent the retention of irritating discharge, and this can only be done by frequently washing it away. Nature

does this for us four or five times daily, but this is not enough; injections should be used, and used frequently. The success attending the use of injections depends principally upon the *frequency* and *efficiency* of their application, and very little upon the nature of the fluid used, provided it is unirritating. Whilst they are regarded merely as local astringent applications they will do but little good; their primary use is to secure *cleanliness*, and they fail entirely in their main purpose unless they free the canal from all secretion. Thus I have found a solution of boracic acid, or of chloride of zinc, one grain to the pint, very efficient, when used often and thoroughly. In the very chronic granular urethritis no doubt the application of a local astringent is useful, but the above remarks refer to the earlier stages of the inflammation. The patient should be directed to raise the organ against the abdomen to get rid of the peno-scrotal bend, and then inject the fluid slowly and steadily, forcing it back quite into the perineum, with his other hand. This should be done as often as possible—six to eight times a day. Frequent micturition should also be encouraged as tending to cleanliness, care being taken to render the urine unirritating by the administration of alkalies. (Mr. A. P. Gould, p. 216.)

Tapering Metallic Stricture Dilators.—Almost all cases of organic stricture admit of permanent relief by dilatation. It is not needful in all cases to endeavour to restore to the full extent the original calibre of the passage. In order to carry out this method of dilatation most effectually, a particular kind of instrument is required. We want essentially a "dilator" and a series of "dilators," each instrument to act on the principle of the wedge, and on that of a series of wedges. We want it to be of weight both in handle and body, and to be capable of taking and maintaining the very highest degree of smoothness and polish. Until the last few years I had employed a series of tapered silver catheters, but I found in many instances their construction too light and slender for my purpose, so that I suggested to Messrs. Salt and Son, Birmingham, the plan on which to make for me a very different set of instruments. These consist, then, of a series of six solid metallic "dilators" of ordinary curve (see woodcut, p. 220.) They are made of the best mandril wire, and are coated with nickel, which not only preserves them from rust, but imparts a smoothness and almost a greasy feeling, much facilitating their use. They regularly taper from heel to point, varying, in a length of nine inches, three degrees of the English catheter scale. Thus, No. 1 represents a gradual increase of 3 to 6, No. 2 of 4 to 7, and so on to the last,

No. 6, 8 to 11. What I venture to claim for the instrument I have described is that it is better balanced than others: that it effects dilatation more gradually, and hence more certainly; whilst its influence, extending over the entire length of the urethra, not only secures dilatation at the crucial point, but affects any secondary narrowings also. The "dilator" should be passed, from the first to the last, in the horizontal position, the patient's head being as low as possible. It should be well oiled, and the urethral opening should be allowed to receive as much oil also as it will retain—the supply in this direction to be renewed when practicable. The first dilator having passed, the further enlargement of the canal may be completed by the introduction of the series either in one sitting or in several, according to the extent of the resistance and the general tolerance of the proceeding on the part of the patient. (Mr. Oliver Pemberton, p. 219.)

Mr. Bartleet, of Birmingham, has devised a modification of Mr. Pemberton's dilating sounds. They are also made by Messrs. Salt and Son. He says:—The sounds I use are graduated to a scale from the point to the centre of the arc, rising 3 sizes, they are then graduated down again to the size they commenced with at the shaft of the sound (see woodcut, p. 222). This I consider an advantage, inasmuch as the shaft held in the hand, and which gives instinctively the size of the instrument used, is really the size of the point or smallest part of the instrument, whilst in many other graduated sounds the point being a No. 2, the shaft is a No. 7, so that while the fingers feel a large sound, the point or part of the sound which may do harm is of the size of a No. 2 or 3; but the graduation down again to the shaft of the instrument renders it in use a bulbous sound, by which you can tell at once when a stricture is passed, a great point in its treatment. (Mr. T. H. Bartleet, p. 222.)

Stricture of Urethra and Urethrotomy.—I would firstly remark that I consider most urethral strictures are best treated by gradual dilatation, carried out by means of soft instruments; that an operation is but seldom called for, and ought, as a rule, to be only resorted to after milder measures have failed. We know that cicatrices are endowed with varying powers of contraction; those, for instance, which result from the clean cut of a surgeon's knife shrink but little, whereas those following lacerations contract greatly. Hence, therefore, a cicatrix made by a clean incision possesses the minimum amount of contraction, whilst that following a laceration has the maximum degree; and, inasmuch as we want a cicatrix which will contract as little as possible, we

must choose a cutting operation, and not a tearing one, like the so-called "immediate dilatation." A good urethrotome ought to fulfil the following indications. 1. It should, when introduced, declare with certainty whether it be in the bladder or not. No urethrotome ought to be used which does not do this, for much discredit has been unjustly brought on internal urethrotomy by surgeons employing instruments which did not prove where they had gone to. Hence, false passages, and even the rectum, have been divided instead of the stricture. 2. The knife should not wound the healthy urethra. 3. The staff of the urethrotome should be very slender, so that it can be passed through very tight, narrow, non-dilatable strictures. 4. The instrument should not only tell where the incision is to begin, but where it is to end. Now I believe that the urethrotome, as modified and improved by Sédillot, Gouley, and myself, fulfils all the above requirements. In Maisonneuve's instrument, the groove in the staff extended through its entire length, so that the knife went into the bladder, which was unnecessary; and, as the slit was usually blocked up by blood or mucus, and hence the withdrawal of the urine, which formed no part of the operation, could not be effected. The groove in my staff is filled with a closely fitting stylet (see Fig. 2, p. 236), so that, when the wire is taken out, urine will flow if the instrument be really in the bladder. (Mr. W. F. Teevan, p. 234.)

Mr. Durham's New Urethrotome.—Mr. Durham operates for stricture with an instrument of a new kind (see engraving at page 228). It consists, in the first place, of a slender steel guide curved in catheter form. This is hollow, and either open at the extremity, for adaptation by screw to a filiform whalebone bougie previously passed after the method of Otis and others, or else closed and rounded at the extremity with openings like those of a catheter for the indicative escape of urine. This guide has four longitudinal grooves (too fine to be represented in the figure) throughout the extent of the straight portion. To facilitate the manipulation of the guide, a small ebony handle is slided on to it and fixed by a screw. When the guide has been introduced through the stricture and the bladder has been reached, as evidenced by the escape of urine or otherwise, the handle is removed, and the main part of the instrument is slided on. This consists of a tube having a hollow cylindrical handle at the proximal extremity, and at the distal extremity an elongated slightly tapering olive bulb in which are four slits. Sliding within this tube is a second, somewhat longer, and furnished at the distal extremity with four sharp-pointed sharp-edged blades;

its proximal extremity is expanded into a circular flat plate, between which and the top of the handle of the outer tube is an adjusting screw; when this adjusting screw is down on the top of the handle, the blades are securely fixed within the olive bulb of the outer tube, and in this state the instrument is slided down upon the guide until the stricture is reached. The whole instrument put together is shown in Fig. 5, with the filiform conductor attached: *a* is the plate of the inner tube, pressure upon which causes projection of the blades; *b* the adjusting screw. The bulb being then firmly applied to the face of the stricture, the adjusting screw is turned up so far as may be deemed desirable. Pressure upon the top plate of the inner tube will then cause projection of the blades through the slits in the bulb, and the strictured part of the urethra will be cut to a slight depth at four points in its circumference. Removal of the pressure is followed by withdrawal of the blades within the bulb by spring action (the spring being in the cylindrical handle of the outer tube). The bulb is then pushed on. If the stricture have been divided to a sufficient extent, the bulb will pass freely on. If it should still be arrested, another and another projection of the blades may be necessary, and can, of course, be easily accomplished. Such projection of the blades is repeated until it is manifest that the strictured part has been sufficiently divided, and the bulb can glide on beyond it. The instrument is then withdrawn and a full-sized catheter at once introduced. (Mr. Arthur E. Durham, p. 227.)

For some time in Leeds this has been treated after Mr. Wheelhouse's plan. The main features of the operation may be briefly epitomised:—1. The introduction of Wheelhouse's straight button-ended staff, grooved to within half an inch of its extremity, down to the strictured point, with the groove looking forwards. 2. The opening of the urethra *in the groove* and *not upon the point* of the staff, so as to secure at least a quarter of an inch of healthy urethra in front of the stricture. 3. The seizing of the cut edges of the wound in the urethra by two pairs of straight-bladed nibbed forceps, while the staff is turned round and withdrawn sufficiently to hook up the upper angle of the opened urethra. A lozenge-shaped window is thus obtained, through which the stricture can be seen, and often the orifice in it can be detected. 4. The introduction of a finely probe-pointed director into the stricture, and through it on towards the bladder. This, of course, is the *crux* of the operation. It is readily to be conceived that, in cases complicated by false passages, the danger of proceeding to cut on the director, upon the supposition of

its having entered the bladder, when it has not, should haunt the mind of the operator; but, as a matter of fact, I think I am justified in quoting Mr. Wheelhouse's expression, that its entrance is "clearly demonstrated by the freedom of its movements." Though difficult cases sometimes occur, I have never seen one in which the director has failed to guide safely. 5. The groove of the director having been turned *downwards*, the stricture is divided on its under surface by the scalpel, and a straight probe-pointed bistoury is afterwards carried along the groove, beyond the external wound, to insure complete division of all obstruction; and then, lastly, a most important and essential step, in order to insure the safe conduct of the catheter into the bladder, Teale's probe-gorget is guided along the grooved director into the bladder, dilating the stricture, proving by the gush of urine which flows along it that the viscus is reached, and forming a metallic floor over which the silver catheter cannot fail to pass safely to its destination. (Mr. Edward Atkinson, p. 223.)

Sugar in the Urine.—*New Apparatus for Detection of.*—The employment of the fermentation-test for the detection of sugar in the urine is, I am afraid, not so frequently used as it should be. This arises from some little difficulty in the manipulation. To obviate this, I have devised a simple and inexpensive apparatus, which is very easy to use, and gives trustworthy results. It consists of a couple of ounce-and-a-half vials, with their corks, and an empty used sardine tin. The lid of the tin is bent at right angles with its cavity, and affords a support to the two vials, an elastic band or two being used to retain them in their proper vertical position, while the cavity of the tin receives a portion of the urine under examination sufficient to cover the inverted ends of the bottles, thus forming an extemporaneous pneumatic trough, allowing the whole concern to be put in any situation where the required temperature can be maintained. The vials are two of the ordinary "long series" ounce-and-a-half size. The corks are of unequal length, and each has a triangular notch, about one twelfth of an inch deep, cut through the entire length of one of its sides. This constitutes the whole of the mechanism. In using, the bottles are to be filled to the very brim with the suspected urine. To the vial which takes the longer cork, a little yeast is to be added; the cork is then forced in level with the neck of the bottle. The notch in the cork allows the superfluous urine to escape. The bottle can then be inverted without a particle of air entering, and placed mouth downwards in the stratum of urine contained in the hollow of the tin. The other bottle is

to be treated in the same way: but no yeast is to be put in it, and it is to be placed side by side with the other. The bottle containing the yeast is recognised by its longer cork; and if sugar be present, at a sufficient temperature fermentation soon commences. Gas is evolved, and is retained in the upper part of the bottle, while an equal bulk of urine is expelled through the slit in the cork. The bottle with the shorter cork having no ferment added to its contents, remains full and unaffected, affording a striking means of comparison. By always using the longer cork for the bottle to which the yeast is added, no mistake can occur. The whole affair, being bound together by the elastic bands, can be safely carried about and exposed to the requisite temperature. The thing is thus done as easily as the copper or other tests. A little modification would afford a quantitative result. Messrs. Salt and Son, of Birmingham, have produced an ingenious little apparatus for this purpose, as suggested by Dr. Birt, an engraving and description of which is given at page 341. (Dr. Thomas Birt, Brit. Med. Journal, March 2, p. 293.)

AFFECTIONS OF THE BONES, JOINTS, &c.

Antiseptic Surgery.—The following is the mode I employ antiseptic dressings. In the first place, I thoroughly cleanse the skin with carbolic soap, even rubbing off the outer layer of cuticle, thus removing all clinging germs. I take care that all instruments, &c., are perfectly clean. After performing the operation, I fill the wound with some antiseptic, generally selecting No. 20 solution of carbolic acid, although many other preparations, such as chloride of zinc, or sulphate of iron, or nitrate of silver, seem to possess equal germicidal powers. I then take pains to render the wound as dry as possible, after which the sutures are introduced and the wound dressed with four or five folds of lint thoroughly well saturated in a mixture of carbolic acid and glycerine. Over all, I place either a pledget of carbolised tow, or dry lint, or cotton-wool. If the subsequent discharge be abundant, I at once liberate some of the sutures, to give free vent to the discharge, and have the wound dressed at least twice a day, otherwise the wound is dressed once a day. I carefully avoid using water in any shape or form, squeezing the matter out, not syringing it out, and wiping with dry, not with wet, lint. It will easily be understood that there are cases where the position of the pus, &c., renders it impossible to remove it altogether by pressure; this, for instance, is the case when the knee-joint, or indeed any joint except the hip-joint, is opened; then it becomes necessary to wash out the discharge

with the syringe. When this has to be done, I am in the habit of using a solution of permanganate of potash in preference to carbolic acid, and take care that the syringing is thorough, and performed twice in the twenty-four hours, so as to give little time for decomposition to take place. Irrigation in this case is good; indeed, is excellent; but it is somewhat messy and involves some exposure to cold, which is not always desirable or safe. There are two other conditions where the syringe comes into play, which I may take this opportunity of alluding to; I refer to the treatment of sinuses and abscesses. Speaking of the first, and supposing that all dead bone, &c., is removed from the bottom of the sinus, and that the granulation-tissue round the orifice is removed with the knife or scissors, I believe the best plan to secure closure of the entire sinus is to thoroughly flush it every day with a mixture of tincture of iodine and water (one to seven). (Mr. S. Messenger Bradley, p. 124.)

Antiseptic Treatment of Wounds.—The dressing that is now almost universally used is Lister's Antiseptic Gauze. To decrease the expenditure, three methods are available: first, to cheapen the gauze dressing; second, to use a cheaper material than gauze; third, to use a more durable material than gauze. The cost price of the charged gauze in the Edinburgh Infirmary Wards is at present $2\frac{1}{4}$d. a yard; and it is difficult to see how the cheapening process can go on much further. I have been using systematically, since 1875, sponges wrung out of 1 to 20 carbolic lotion, and applied over the deep dressing before the application of the outer dressing; by this means I have been enabled to lessen materially the quantity of gauze used at each dressing. I have further been enabled to dress the wounds less frequently than before. The sponges improve with use. If obtained at wholesale prices from dealers in sponges, and if small sponges are used, they can be obtained at a remarkably cheap rate. The authorities of the Royal Infirmary of this city obtained for me, for 3s. 6d., 60 small sponges weighing 1 lb. The smaller the sponges, the more easily they can be applied. These sponges may be stitched together, forming a layer; or they may be laid singly on the deep dressing, and held in position by the outer dressing. Before application, the carbolic lotion must be squeezed from the sponge. The sponge is applied practically dry. The channels in it by capillarity suck up the fluid discharges; and if a catgut or horse-hair drain is used, the sponge may be looked upon as a direct continuation of the catgut or horse-hair drain; or if an indiarubber drainage tube is used, the power of the sponge may

be likened to the suction power of a syringe on drawing up the piston. *Is there any way in which the surgeon may dress his wounds without the constant aid of the spray producer?* In several of my cases a permanent deep dressing was applied on the day after the operation, and fixed in position either with a bandage or with some sticky material, such as Canada balsam, or a solution of guttapercha in chloroform. From the experience I have had in these cases, I am of opinion that if the dressing is so arranged as to be perfectly porous, and if an absorbable method of drainage is used, as catgut, it will not be necessary to remove the deep dressing until the wound is superficial. As long as the deep dressing is in position, the spray will not be required. All that is necessary is to remove the outer dressing when the discharge reaches its edges; to damp with carbolic lotion and salicylic paste the deep dressing, and to apply anew an external dressing. It must be remembered that the deep dressing has lost its antiseptic qualities, while it remains as long as it is covered by the outer dressing, perfectly aseptic. It must, therefore, be thoroughly damped with carbolic lotion whenever it is exposed to the atmosphere, in order to destroy any mischief that may have fallen upon it during the exposure, and in order to render it actively antiseptic, so that when the dry gauze dressing is applied over it, no mischief may pass from it through the deep dressing into the wound. The spray is used at the operation and at the first dressing, and afterwards only when the deep dressing is removed. I have found, as yet, a gauze bandage the most suitable method of fixing the deep dressing on the limbs. This method is therefore available in all operations on the limbs. (Mr. John Chiene, p. 133.)

Thymol.—The New Antiseptic.—A rival to carbolic acid has certainly been discovered in thymol, the essential ingredient of oil of thyme, which is prepared either by treating the oil of thyme itself with a strong alkaline solution, skimming off the thymene and cymol, which separate and rise to the surface, and precipitating the thymol which remains in solution with hydrochloric acid; or else (and this appears to be its most common commercial source at present) by distilling the seeds of *Ptychotis ajowan*—an East Indian umbellifer, which contain from 5 to 6 per cent. of their weight of this body. The following is the best formula for the preparation of thymol for antiseptic purposes:—thymol, 1 gramme; alcohol, 10; glycerine, 20; water, 1000 grammes; or in the same proportions. This solution has no irritant properties. It may be used as spray. Lister's gauze-bandages may be pre-

pared with thymol instead of carbolic acid; but in using them it is not necessary to cover the wound with "protective," as thymol, unlike carbolic acid, is quite unirritating. Thymol is much more costly than carbolic acid, but this is made up for by the small amount of discharge from the wound requiring a less frequent renewal of the bandages. (Editor of Medical Times and Gazette, p. 137.)

The New Antiseptic Thymol Gauze.—While appearing to be quite as trustworthy an antiseptic as carbolic gauze, it is free from the objections of stiffness, of irritating action of the skin, and of the disagreeable odour of the carbolic acid. It readily permits transudation of any fluid escaping from a wound, but does not appear to increase any secretion, nor to irritate the edges of a wound, nor the skin surrounding it. The wound, as a rule, does not require dressing until it is time to remove the sutures, and one more dressing is all that is required. A solution of 1 part of thymol in 1,000 of water appears to answer well for irrigation, for spray, and as an antiseptic bath for instruments and for sponges. ["A solution of thymol in water of a strength of 1 in 1,000 is all that is required. Thymol is soluble in warm-water in that proportion, and there is no separation on cooling. As your readers may wish to know the price at which this gauze can be supplied, we may add that at present it is sold in packets, of six yards long by a yard wide, at 3s. 9d., or 8d. per single yard. When manufactured on a large scale, some reduction may possibly be effected. Thymol itself cannot be sold here for less than 2s. 6d. per ounce. Calvert's carbolic acid, No. 1, is 6d. per ounce; but if, as is expected, a solution of 1 thymol in 1,000 prove as efficacious as 1 carbolic acid in 40, the relative cost is in favour of thymol in the proportion of 5 to 1. It may interest your readers to know that we have prepared for Mr. Wells an adhesive plaster, containing 1 part of thymol in 1,000 of plaster, which appears likely to fulfil the desire so often expressed of a non-irritating antiseptic adhesive plaster." —Messrs. Squire.] (Mr. T. Spencer Wells, p. 141.)

Chronic Bursitis.—*Antiseptic Treatment.*—In a case of this kind, the skin having been purified with strong carbolic lotion, a small incision was made with a tenotomy knife into the bursa under the spray, and about half an ounce of serosanguineous fluid was squeezed out. A few threads of fine carbolised catgut were then introduced with sinus forceps, and the usual gauze dressing was applied, the limb being bandaged to a posterior splint. The dressing was left untouched for ten days, during which the patient never complained of pain, and on its removal on the tenth day the

bursal swelling was found to have entirely disappeared, the tiny wound was completely cicatrized, and the portion of the catgut skim outside the wound was lying on the skin just as it had been left, but cut off by the cicatrix at its point of exit so that it could be rubbed away with the finger. (Dr. R. Roxburgh, p. 136.)

Compound Fractures. — *Antiseptic Treatment.* — *The first dressing decides the fate of the patient and the course and issue of the wound.* All counter-incisions must now be made and drainage-tubes put in; loose splinters of bone must be removed, and the fractured extremities put into position; any little sharp projections may be rasped away; and the wound then must be completely disinfected. If this is done thoroughly and with care, the knife need never be taken into the hand again, however long the healing may require; nor will any further drainage-tubes be required, though possibly some small bits of necrosed bone may require removal; but this should only be done when all danger to the patient is past and over. Here is ample ground for doing the first dressing with the very greatest care, and with the most pedantic minuteness. We must not hesitate to bestow half an hour, or even three-quarters of an hour if so much be necessary. The wound may once again be well washed out with carbolised water, and the remaining part of the dressing must be carried out under the carbolic spray. First of all, the wound itself and its surroundings are covered with a thick handkerchief-like pad of carbolic gauze. I prefer this to the "protective silk" for the first few days; for the fifty to a hundred layers of gauze which are thus lying on the wound readily absorb both any blood and wound secretions which may flow out. Then upon this comes Lister's dressing proper, which I need not here further describe. I generally change this first dressing on the following day, or on the day but one after at latest, in order to see whether all is going on properly, and whether the drainage tubes are *in situ* and acting properly. Subsequent dressings are done every second, or third, or fourth day, according to circumstances; so soon as there is no further secretion from the wounds, after freely squeezing the limb, I remove the drainage-tubes. This is generally done about the third or fourth day. The antiseptic dressing must be continued until the coagula, filling up the wounds, have become organised, or until their place is taken by granulation tissue. (Prof. R. Volkmann, p 153.)

Hip-joint Disease.—*Extension with Motion.*—We are indebted to the American surgeons for the discovery of two very important principles, and also for their practical application by

means of most ingeniously contrived instruments. *The first principle* is that of extension, as a means of relieving the most acute pain in joint-diseases, especially applicable to the knee and hip-joints. *The second principle* is that of extension combined with motion during the progress of disease, the patient being allowed to walk about, so as to promote recovery with free motion in the joint, instead of the ordinary result of ankylosis obtained by long-continued rest and immobility. There can be no doubt that the discovery and practical application of these two principles have completely revolutionised the treatment of joint-diseases, and changed our opinion with regard to the pathological conditions existing, especially as to the production of acute pain, which formerly was believed to depend upon acute inflammation, requiring active local, as well as general antiphlogistic treatment, such as leeches, blisters, calomel and opium, &c. It has now been proved to depend upon undue articular pressure and contact of inflamed surfaces, produced by reflex muscular contraction, and capable of relief by mechanical means alone, producing extension, whether this be applied by means of the weight and pulley, or by the screw and cog-wheel. The object of extension is not, as generally supposed, to separate articular surfaces, but to overcome reflex muscular contraction, and, by relaxing the muscular rigidity, to prevent undue pressure of inflamed articular surfaces, or their margins, when the joint is held in a flexed position by muscular contraction. The successful application of weight-extension as a remedy for pain, however acute it may be, in joint-disease, is the greatest discovery of modern times in the treatment of these affections. The originality of this discovery is claimed by Dr. Henry G. Davis of New York. The English idea has always been rest and immobility to the joint. The American idea, during the last ten years, has been extension with motion, *i.e.*, preserving motion in the joint whilst the pain is relieved by extension. (Mr. William Adams, p. 147.)

Horse-hair for Drainage of Wounds.—It may frequently happen that the most dependent part of a wound may have no opening in the skin to correspond with it: thus after excision of the mamma it may turn out, when the operation is concluded, that the wound presents a pocket extending considerably further back than the outer angle of your incision. Under such circumstances it is desirable to make an opening for the exit of the drain at the most dependent part. Now, if this were done by a puncture with the knife, some arterial branch of considerable size might be wounded, involving the necessity of freely enlarging the wound to

secure the bleeding point. But if you take a pair of dressing forceps, and bore steadily from within outwards, the conical extremity of the instrument will slip past any arterial branch or nervous trunk without injuring it, and when at length it is apparent that there is nothing but skin between the instrument and the surface, the tough integument is divided with a knife over the point of the forceps, and the blades being forcibly expanded so as to enlarge somewhat by laceration the opening which has been made in the muscles, or other deeper textures, the drain is seized between the blades of the forceps, and drawn into place. While the horse-hair has the advantage over catgut that it can be used when necessary over a longer period, it has, in some cases, the converse superiority that it can be not only reduced in bulk, but withdrawn altogether at an earlier period than is required for the absorption of the catgut; for the catgut, in process of organisation and absorption, becomes more or less incorporated with surrounding tissues through the medium of the cells of new formation which invade it, and, if an attempt is made to withdraw the drain in whole or in part, there will often occur inconvenient oozing of blood through the rupture of newly formed vessels. (Prof. Lister, p. 142.)

Scalp Wounds.—Anybody who has seen an ordinary scalp wound will have noticed how freely it bled. The blood spurts out; the hemorrhage is profuse and difficult to control. What is the reason of this? If you look to the structure of the scalp, you will find two or three points that will explain it. In the first place, the scalp is extremely vascular; it is supplied with many blood-vessels, which anastomose freely through it, and not only is the vascular supply unusually free and abundant, but these vessels are serpentine. You will see on the scalp of old people, when the fatty matter has become absorbed, the serpentine course of the arteries mapped out very plainly. The amount of vascular supply will explain the hemorrhage; but to what are we to refer the frequent uncontrollable character of the hemorrhage? It is to this: these vessels run between the skin and the tendon of the occipito-frontalis; they run in a quantity of very dense granular fat, which lies between the skin and occipito-frontalis. In consequence of lying in this dense granular fat, you will find that these vessels, when divided, do not retract, and they cannot contract. (Prof. J. E. Erichsen, p. 161.)

Reparative Power of the Scalp.—The scalp stands in quite a different category to other portions of the integument. You may remove portions of the scalp, and cicatrisation takes place very rapidly indeed. This will explain the good results

that follow when a nose is made from the scalp by the Indian operation. Although a large portion of the scalp is turned over from the forehead, very little scar is left. In this operation we have a good illustration of the reparative power possessed by the scalp. I have seen, in a person who had fallen into the fire in an epileptic fit, half the scalp removed from the side of the head, and a portion of bone detached by a process of exfoliation. But the wound contracted, and left a firm cicatrix. (Prof. J. E. Erichsen, p. 164.)

Erysipelas of the Scalp.—There is one great anatomical peculiarity that leads to the more frequent occurrence of erysipelas here than elsewhere—namely, that the occipito-frontalis tendon lies upon a plane of very loose and lax areolar tissue, which intervenes between it and the pericranium; and if that layer of areolar tissue be laid open, you are very apt indeed to get profuse suppuration there, and you get erysipelas of the scalp—you get a kind of cellulitis, which renders wounds of the scalp that penetrate deeply very dangerous. There is, in point of fact, this difference between erysipelas of the scalp affecting the skin only—the idiopathic,—and that implicating the occipito-frontalis between it and the pericranium—the traumatic. In ordinary idiopathic erysipelas of the scalp death scarcely ever occurs. I think I can scarcely recollect ever having seen a person die from erysipelas of the scalp that was not traumatic. Death of course may occur, but it is extremely rare. It is very different with traumatic erysipelas of the scalp, which is extremely dangerous and very often fatal. (Prof. J. E. Erichsen, p. 162.)

Sebaceous Tumours of the Scalp.—In operating upon one of these tumours, it is better simply to split its upper part, and not to divide it down to the base; then pull it out by its capsule without dividing it completely through. But if you divide the base and detach the two parts separately, it will be very difficult to get it out clean, and may require a dissection when it would otherwise have been unnecessary to do so. In cases of injuries of the scalp, where the hemorrhage is difficult to arrest, how are we to stop it? We may always stop it at once by acupressure. You can always pass a harelip pin under the bleeding point without any difficulty. (Prof. J. E. Erichsen, p. 161.)

Stumps after Amputation.—*New Method of Treatment.*—My manner of procedure is the following. We shall suppose an amputation at the wrist. I apply the tourniquet over the brachial artery; I cut my flaps very carefully, that they may adjust as closely as possible; and I bring the divided parts

together, and keep them in apposition by means of strips of linen one inch in width, soaked in a solution of equal parts of tincture of muriate of iron and water. I lay my strips first horizontally, and then spirally, using moderate and uniform pressure so as to prevent subcutaneous oozing of blood, and I further saturate the compresses with iron. I now slightly turn the screw of the tourniquet to allow of a little blood to reach the bandages. The blood coming in contact with the iron undergoes a chemical change, and forms a thick adhesive mass, which closes the lips of the wound, and excludes all contact of air. Shortly afterwards I remove the tourniquet, when no hemorrhage can take place owing to complete closure of the wound and through compression over the veins and arteries. To ensure the latter effect more thoroughly I previously envelop the limb up to the elbow with rollers of bandage firmly and moderately placed from below upwards. As regards the use of the tourniquet, perhaps it would be better still to substitute Esmarch's elastic bandages. The points of practical importance gained by the method I submit are the following. The wound heals by first intention; the healthy living tissues uniting without suppuration, or, in other words, no "putrefactive fermentation" takes place, just the same condition—the aseptic—as claimed for Professor Lister's method; the non-use of ligatures and sutures, a frequent cause of septic mischief: and last, though not least, its simplicity and astonishing results. (Dr. E. Gaurveau, of Quebec, Lancet, Jan. 5, p. 31.)

Wounds.—The most simple application for sealing up wounds is the old-fashioned tincture of benzoin, and it is the most successful. By it nearly all fresh wounds heal rapidly, while they do not do so under watery and fatty dressings. Tincture of benzoin has a remarkable property of uniting tissues and combining with blood. It is antiseptic, and, assisted by cotton-wool pads of lint and firm bandaging, will arrest hemorrhage from all vessels less in size than the radial artery. Non-recent wounds which suppurate it is not desirable to heal by adhesion. The most important item in the treatment of these is ventilation with as pure air as possible. None but the most evil results follow the application of waterproof materials such as oiled silk and gutta percha tissue over the dressings. Such wounds invariably stink and slough; the wound is made unduly hot, products of decomposition are retained, the surface has a greyish grumous aspect and loses substance daily. A simple piece of lint or muslin covered by cerate, or dipped in lotions of Condy's fluid (1 to 40), or tincture of myrrh and water (1 to 20), spirit and water or weak

carbolic acid lotion (1 to 60), with just a layer of bandage to retain the dressing in its place, is all that is necessary, save a daily syringing and washing with warm Condy's fluid and water. (Mr. Philip Cowen, p. 168.)

AFFECTIONS OF THE SKIN.

ACNE.—The basis of all treatment should be vigorous rubbing with soap and flannel, for friction with soap, more than anything else, prevents the formation of comedones, and consequently of the acne pimple. The following plan of treatment succeeds in a large number of cases:—(1) The face should be steamed every night by holding it over a basin of hot water for a few minutes. (2) The skin should then be well rubbed for five or ten minutes with soap and flannel, or a soft nail-brush may be used with advantage when the skin will bear it; the soap should then be sponged off with warm water. (3) When the face has been dried, a lotion, composed of half-an-ounce of precipitated sulphur, two drachms of glycerine, one ounce of spirits of wine, three ounces each of lime-water and rose-water, should be thoroughly applied and allowed to dry, and remain on all night. If the skin is greasy the addition of some ether to the lotion is an advantage. Sometimes an ointment is more effective than a lotion; in that case one drachm and a half of hypochloride of sulphur, ten grains of carbonate of potash, ten drops of oil of bitter almonds, and an ounce of lard may be used; or three drachms of sulphur ointment and five drachms of vaseline will be found to be a very useful unguent. Whatever is used should be allowed to remain on all night, and washed off in the morning with warm oatmeal and water or weak gruel. If the skin becomes very tender under this treatment, it may be discontinued for one or two nights and then resumed. The most common cause of failure is want of perseverance or timidity on the part of the patient or doctor, for a temporary increase in the redness and irritability of the skin often prevents the continuance of the most efficacious remedies. The treatment I have here indicated will be generally successful in dealing with ordinary acne, but will fail in a certain proportion of inveterate cases. In these I find nothing so effectual as the application of potash soap in the form of a lotion well rubbed on the skin every night. One ounce of soft soap, one ounce of rectified spirits of wine, and six ounces of rose water will generally be found of sufficient strength. The lotion should be applied with a piece of flannel, and vigorously rubbed on the skin for as long a time as is convenient, the longer the better, short of making the

skin really sore. Then the lotion should be washed off, and one of the sulphur lotions applied and allowed to dry on. Sometimes, after the vigorous use of soft soap, sulphur is not easily borne; in that case the following lotion may be used instead:—Prepared calamine powder and oxychloride of bismuth powder two drachms of each, rectified spirits of wine half an ounce, glycerine one drachm, perchloride of mercury three grains, and rose water to eight ounces. The quantity of perchloride of mercury may be increased if necessary. The worst cases of acne will in time yield to the soft soap treatment. (Dr. R. Liveing, p. 191.)

Chronic Ulcers.—*Iodoform.*—Iodoform is readily obtained by adding an alcoholic solution of potash to tincture of iodine, and crystallises as a yellow lustrous coarse-grained powder of a peculiar pungent penetrating odour. Ulcers that have remained open for years, and on the treatment of which much care and skill have been expended, often close in a few weeks under its influence; but the same caution must be repeated as in the case of venereal sores. It will only irritate the actively inflamed wound. It is the indolent ulcer, from whatever cause it may arise, whether from varicose veins, malnutrition, syphilis, or injury, that is especially benefited by iodoform. Repeatedly, under its use, I have seen a surface, glazed or œdematous, rapidly take on healthy action, granulate, and heal, and this where other measures have been tried for months, or even longer, without effect. Often, too, the pain that so frequently accompanies these ulcerative processes ceases after iodoform has been applied for a few hours. As to its mode of application. If used as a powder, iodoform should be dusted on the ulcerated surface, and a piece of dry lint, or lint soaked in a weak solution of carbolic acid, may be laid over it, and this process repeated night and morning. Undiluted, I have often found it apt to produce irritation and pain, and, therefore, generally prescribe it mixed with equal parts of either fullers' earth or tannin. As a parasiticide, I have used it as an ointment with about twenty grains to an ounce of lard, and have directed it to be applied twice daily. Such an ointment spread on lint is a convenient mode of application to a wound or ulcer, and its employment in this form prevents the risk of dropping this disagreeably smelling drug on the patient's clothes, &c. If an ointment of the strength named cause inflammation or pain, it may be diluted. I am also in the habit of ordering iodoform in combination with a salt of mercury, &c., with satisfactory results. So also it may most conveniently and easily be applied by painting the part with its solution in

alcohol, chloroform, or ether, as Mr. Hill describes. There are two drawbacks to the use of iodoform. The first is its extremely disagreeable odour, which, unless it is carefully covered over, scents the room in which the patient is; the second is its high price. The latter, however, would soon be reduced, if any considerable demand arose for it. (Dr. W. Cottle, p. 337.)

Fatty Tumours.—*Diagnosis.*—The diagnosis of fatty tumours is not at all times easy, but a method has been suggested of solving the difficulty, which consists in applying ether or ice to the part, in the case of a doubtful tumour. If the growth is felt to become harder, the presumption is that the tumour is fatty. (Mr. Francis Mason, Lancet, Jan. 19, p. 77.)

Psoriasis.—*Phosphorus and Chrysophanic Acid.*—It was decided to treat a most intractable case of this disease with chrysophanic acid ointment and phosphorus "perles." The full effect of phosphorus in psoriasis, I knew by previous experience, would take a month to assert itself. The effect of chrysophanic acid would, as I knew quite positively, be declared for yes or no, within a week. The patient was treated with chrysophanic acid ointment of the strength of two drachms of the acid to the ounce of lard, the former being fully digested in the latter at a temperature of 360° Fahr. (oil bath), to insure the requisite incorporation of the acid with the lard. The phosphorus, exhibited in the form of "perles" —that is to say, the little capsules containing each one-thirtieth of a grain of phosphorus dissolved in oil, which may be obtained of Messrs. Corbyn, or, indeed, as I believe, of almost any chemist,—was regulated in the first instance to one-tenth of a grain per diem, but was speedily increased to rather over a third of a grain per diem, which proved to be the greatest dose that the patient could tolerate without experiencing gastric pain. The patient was taught to soften his scales efficiently with soap and water, and then to remove them thoroughly by scraping them away with a dull-edged knife before each application of the ointment. I began treatment on September 24, 1877. By October 1, the greater part of the eruption had disappeared. (Dr. B. Squire, p.195.)

Two cases of general psoriasis were treated in Charing Cross Hospital on the following plan:—The trunk and limbs were washed night and morning with soft soap, the latter being used pretty vigorously over the diseased patches. Afterwards, a weak ointment of chrysophanic acid (five grains to the ounce, in one case seven grains to the ounce) was rubbed on the affected parts. A tight-fitting linen vest was

worn next the skin, and the limbs were bandaged. No inconvenience was experienced, excepting in one case a trifling erythema, or, as it seemed rather, a false erythema, produced by staining of the epidermis. The patients were kept in bed. There was intolerance of arsenic in both cases, so that a carbolic acid mixture thrice daily was ordered. Both patients remained under the above treatment about a month, and left the hospital "cured." (Dr. Alfred Sangster, Lancet, March 16, p. 402.)

Ringworm of the Scalp.—*Iodoform.*—In many cases of ringworm of the scalp of long duration, and which had been before the subjects of much and careful treatment, I have prescribed iodoform in the form of an ointment. In several of these, speedy improvement ensued, spores being no longer to be found and the parts returning to a state of health; but I met, in some instances, with considerable difficulty in inducing the parents to apply the remedy, on account of its powerful odour. It set up no violent inflammation, and I hope it may prove an useful adjunct to the means at our disposal for combating that disease. (Dr. W. Cottle, p. 337.)

Thymol as a Remedy in Skin Diseases.—Thymol is obtained from the essential oil of thyme, which is found in several plants. I have used the following formulæ:—1. An ointment, consisting of one ounce of vaseline and from five to thirty grains of thymol; the thymol being dissolved in the vaseline. 2. A lotion, consisting of thymol, five grains; rectified spirit and glycerine, each one ounce; water sufficient for eight ounces. The glycerine is added to correct the desiccating effect of the spirit alone. 3. A solution of five to eighty grains of thymolate of potash in eight ounces of water. As yet, I have not had occasion to use stronger lotions than the above. I have only lately used the last lotion; but so far, have found it equally efficacious, while it has the advantages of economy and the readiness with which the strength may be increased. Ointments made with lard instead of vaseline act very well, but vaseline ointments have a better appearance. The disease in which I first prescribed it and have had the greatest success is psoriasis. I found it better to begin with a weaker ointment, namely, ten grains to the ounce; and then, if the remedy were suitable, to continue as long as improvement was manifested, and if it became stationary, to increase the strength by five grains to the ounce until, in some cases, thirty grains to the ounce was reached. It is most successful in that class of cases in which tar is usually prescribed, and while quite as efficacious and in some cases succeeding where tar fails, it is cleaner, colourless, and hence

can be used on the face without producing the brown discolouration of oil of cade and other preparations of tar, while the odour is rather pleasant than otherwise. In the later stages of eczema it is also extremely useful; some cases of very long standing, which had been submitted to other treatment of various kinds, rapidly yielded to thymol. It was necessary in eczema to use a weaker ointment of only three to five grains to the ounce; and I have not met with any case of eczema that required a stronger application than that, and unctuous are generally better than watery applications in this disease. As might be anticipated, it is adapted to a smaller proportion of cases than psoriasis, and must be restricted to cases in the dry stage or where the amount of discharge is diminishing, *i.e.*, not until the activity of the inflammation has subsided; hence it happens that even in the same patient it would cure one part, and be too stimulating for another part where the inflammation was still active. If, however, due discrimination be employed, the duration of the disease may be much curtailed. Smarting when first put on is rather more frequent than in psoriasis. With similar precautions, it also rapidly completes the cure in so-called lichen agrius: but usually a preliminary soothing treatment is required for some time before thymol is prescribed. (Dr. H. R. Crocker, p. 199.)

VENEREAL DISEASES.

Calomel Vapour Bath.—It is twenty-three years, Dr. Yandell remarks, since he commenced the use of the mercurial vapour bath, and he has used it ever since. I now use calomel that has been previously resublimed two or three times. Ordinary calomel is less affected by heat or moisture than any other preparation of mercury, but still it does contain a certain amount of hydrochloric acid, the presence of which may be indicated by a piece of moist litmus-paper held in the fumes as they arise. This free hydrochloric acid is driven off in a great measure by sublimation, and the pure calomel thus prepared is less irritating than the ordinary calomel of commerce. It should also be observed that the water I originally used was principally for the purpose of preventing irritation from any fumes that might be generated during the action of the baths, and I find that an ounce on each occasion is quite sufficient. If more water be employed, more heat is necessarily required in order to boil it. The vapour of the water is in part deposited on the patient's skin; this must in some way be removed before he is comfortable, and some of the calomel is necessarily removed with it. Dr. Yandell uses a

pint of water in the apparatus which he has depicted, and the patient has thus a combined vapour and calomel bath. This no doubt may be very useful where such a combination is intended, but the effect is often very different from that produced by the calomel bath alone; a much greater amount of perspiration is induced, and this the patients, when the bath is repeated night after night, cannot bear. The perspiration also tends to remove the calomel from the skin. Dr. L. P. Yandell is of opinion that brisk friction after the sweat, made with the coarsest towel, and until the skin is all of a glow, actually promotes the action of mercury, and conduces to its more rapid absorption. This no doubt may be the case, but it involves a different principle. The calomel is rubbed into the skin in a similar way as the mercurial ointment was in olden times. (Mr. Henry Lee, p. 255.)

Syphilitic Ulceration of Palate.—*Iodoform.*—In specific ulceration of the soft palate, pharynx, tonsils or nasal passages, iodoform will often be most efficacious. In those indolent cases which have been believed by able observers to be scrofulous ulceration, but which some of their critics still think due to syphilis, iodoform will sometimes bring about a favourable change when the failure of other means has been most discouraging. As it is equally useful in unquestioned scrofulous conditions, the amendment cannot be used as a diagnostic aid. In ulceration which can only be detected by rhinoscopic examination, the iodoform may be applied by the assistance of the mirror. In this way, some most obstinate cases of ozæna and other troubles may be cured. (Dr. Prosser James, Brit. Med. Jour., Feb. 9, p. 193.)

Venereal Sores.—*Iodoform.*—Locally, iodoform, as a dry powder, brushed lightly over the surface with a moistened camel-hair pencil, has been for three years my almost invariable treatment of venereal sores, especially the local chancre. During the last few months I have often substituted for the dry powder an ethereal solution (one part of iodoform in six or eight of ether). The sore is touched or dabbed with a pencil dipped in the ethereal solution, according to its size and depth, lightly or copiously. The ether quickly evaporates, leaving a thin pellicle of iodoform, that as effectually stays the spread and produces healing of chancres as does the more copiously applied dry powder. Thus the surface is covered more exactly, and the disagreeable smell of the iodoform is too faint to attract attention. The sore is well washed with water and dried before the iodoform is applied, and the surface is lastly protected by a bit of dry lint. When the secretion is abundant, the dressing must be renewed twice daily,

but in three or four days the amount of discharge becomes so scant that one dressing *per diem* suffices. In this way venereal sores heal quickly. Pain subsides at once; the sore is well in a week or ten days, and the chances of consecutive inoculation or bubo are greatly lessened. In a very few cases, the application of iodoform gives momentary smarting, which is very bearable; even the ethereal solution does not hurt, and usually the patient declares the application to be quite painless. I avoid using iodoform on inflamed sores, or on simple granulating wounds; but indolent non-specific ulcers are rapidly improved by iodoform locally applied. (Mr. Berkeley Hill, p. 253.)

DISEASES OF THE EYE AND EAR.

Mydriatic Properties of Duboisia Myoporoides.—The duboisia is a shrub growing plentifully in Eastern Australia. An extract of the plant had been observed to dilate the pupil freely, when applied to the eye. Dr. Ringer reports that "the physiological action of the extract of duboisia is apparently identical with that of atropia. The same remark applies to the effects of the local application to the eye. If there be any difference, it is that duboisia is more prompt and energetic than atropia, and certainly very much more so than the strongest extract of belladonna." (Dr. Sydney Ringer, p. 340.)

Tympanic Cavity.—*New Method of introducing Air into.*—One of the chief ends we try to attain by the introduction of the catheter is the passage of air through the Eustachian tube into the middle ear, a method of treatment lately made known under the name of air-douche, and notoriously more frequently employed than any other in the treatment of ear diseases. Even for using the air-douche it was desirable to learn the use of the Eustachian catheter, though the same, or almost the same, end might be attained without the actual passage of the tube. The method of accomplishing this end employed of late years is that first described by A. Politzer, which consists, as is well known, of the following steps. The patient takes a little water in his mouth, the surgeon passes the nozzle of the specially-made instrument about one centimetre within the nasal opening, which is firmly closed upon the instrument with the thumb and forefinger of the left hand; then, while the water is being swallowed (at a signal from the surgeon), the elastic ball attached to the nozzle in the nasal opening is suddenly emptied by pressure with the right hand of the surgeon. I found many drawbacks in the use of the swallowing movement, as employed

by Politzer, I devised a substitute, which I believe to be better for our purpose. I will now proceed to a description of the process itself. As has been already said, in order to obtain an effective separation between the upper and lower parts of the pharynx, the muscles of the soft palate must be brought into play at the same moment that the Eustachian tube is opened. All this is obtained by the simple pressure of the root of the tongue upon the hinder part of the palate, if a strong expiration is made at the same moment. If one presses the posterior part of the tongue against the palate, the cavity of the mouth is shut off from the throat, and the soft palate is pressed upwards and backwards. The air, which passes in expiration into the throat, has no escape either through the mouth or through the nose, of which fact one can easily convince oneself by holding the hand, or a small flame, in front of the nose. The latter is not moved, and the hand is not conscious of the least breeze during the expiration, as would be the case did the air escape from the nose. The stronger the expiration at this moment, the more tense will be the soft palate by the pressure of the escaping air, and the more effective the closure of the upper pharynx. This moment, as regards the arrangement of the pharyngeal parts, is the most favourable for giving the maximum degree of pressure to the pent-up air, by emptying the Politzer ball into the nose by the nozzle introduced as usual. (Dr. J. Gruber, p. 246.)

MIDWIFERY, &c.

After-Pains.—If after-pains are a continuation and supplement, as I believe them to be, of the normal parturient process, then the less we meddle with them the better. This expectant method I have found in the great majority of cases to be both safe and strictly correct. It is only when they continue unusually long, and are very severe, that remedial measures are called for. Here I have found small doses of opium frequently repeated, when no uræmic complication co-existed, combined with an alkali or acid, as either was indicated, to answer an excellent purpose. This plan I consider much safer than giving the drug in a single large dose, which has a tendency to suddenly paralyse nervous action, and to subsequently lead to passive enlargement and congestion of the uterus. (Dr. Bernard Kelly, p. 261.)

Fibroid Tumours of the Uterus.—The drug that most powerfully and unmistakably affects the growth of fibroid tumours of the uterus is the ergot of rye. Its influence on the developed muscular fibres of the uterus naturally led to

its employment in cases of fibroid tumours with hypertrophy of the surrounding walls; and the concurrent testimony of many gynecologists puts the action of ergot in the treatment of these growths among the best established phenomena of therapeutics. Professor Hildebrandt, of Konigsberg, demonstrated the safety and certainty with which an active dose of ergotin could be administered hypodermically. He showed what my own experience, as well as that of Byford, of Chicago, and others has amply confirmed, that the repeated subcutaneous injection of from 2 to 5 grains of ergotin can be counted on with great certainty to excite appreciable contractions in the walls of uteri in which the muscular fibres have become hypertrophied. The preparation of ergotin which I have found most satisfactory is the same which I brought under the notice of this Society in treating of the complete evacuation of the uterus after abortions. ℞. Ergotinæ, ʒ ij; aquæ, ʒ vj; chloral-hydratis, ʒ ss. M. Twelve drops of the solution or rather mixture—because the ergotin is partly dissolved and partly suspended—gives a dose of three grains, and this may be regarded as a medium dose, to be administered daily, or every second day, or twice a week after the influence of the drug begins to be manifested. In making this hypodermic injection, it is necessary to take care, 1st, that the fluid carry with it no small globules of air; and, 2nd, that the point of the syringe be carried deeply down through the skin and areolar tissue, right into the muscular strata. Sometimes the injection may be made in the abdominal walls; in most cases they are borne best in the gluteal regions. I cannot understand how the practice has crept into our hospitals, but I observe that when students are called to make such an injection they pinch up the skin and push the point of the needle obliquely through, and occasionally to some distance among the cellular tissues beneath the skin. Now the pinching up of the skin may do good and serve to make the surface somewhat tense; but the needle should certainly always be carried in as perpendicularly to the surface as possible, and straight down with one quick stroke into the muscular tissues. Such a preparation so introduced, is not liable to be attended with the suppurations which have deterred some practitioners from the continuance of this mode of administration. It is but rarely even that the patients complain of the pain. I can only recall two out of the many patients in whom I have used it, who objected to the frequent repetition of the injection on the score of the local suffering. They may be made daily, or every second day, for several weeks; or after some frequent injections for a month they may be continued once or twice

a week for many months without producing any constitutional disturbance. Hildebrandt has correctly indicated the condition most favourable for the use of ergotin in stating that the tumour must be intramural or submucous; in other words, it must be surrounded by layers of muscular fibre, sufficiently developed to be capable of being excited to contraction, and sufficiently powerful to exert some degree of pressure upon the body in their embrace. (Professor A. R. Simpson, p. 279.)

Hemorrhage after Labour.—When you cannot arrest the flooding by other means try Dr. Atthill's plan described by him as follows. She was in a state of great danger, and in a condition which would have warranted the use of the perchloride of iron, but instead of having recourse to it, I resolved to inject hot water; this was procured in a moment, and passing the tube of the syringe right up to the fundus of the uterus, I injected water freely at the temperature of 110°, keeping my hand at the same time over the fundus. I was pleased to find that the uterus contracted firmly under it, exactly as it would close had I employed the perchloride of iron. In a very short space of time, probably before I had injected more than a pint of the hot water, the fluid ran nearly clean from the vagina, the pulse improved markedly, and I ceased to inject any more. After a short time the binder was applied; no further bleeding occurred, and the patient made a rapid and good recovery. The cases I now record undoubtedly establish this much—that the injection of hot water powerfully stimulates the uterus to contract, and thus rapidly checks the hemorrhage; but that it does more is, I think, as clearly established; it evidently acts as a general stimulant. The effect on the pulse was most marked; indeed the pulse was affected more rapidly than by the hypodermic injection of ether, and it did not flag again. The faces of the patients, too, lost the deadly hue they previously had worn; and last, not least, they expressed themselves as having experienced the greatest relief, and obtained great comfort. I anticipate very good results from the introduction of this simple treatment into obstetric practice. It should be remembered that the advantage to be derived from the intra-uterine injection of hot water is not confined to cases of post-partum hemorrhage. It was first used to check hemorrhage occurring in cases of chronic disease of the uterus, and after operations. Water in which the hand can be kept without discomfort may, with safety be employed; but, it must be remembered, that if the temperature be allowed to fall much under 110°, disappointment will certainly follow; equally will the injec-

tion be well-nigh useless if the tube of the syringe be not passed right up to the fundus of the uterus, or at least fairly to within its cavity. (Dr. Lombe Atthill, p 318.)

Inversion of the Womb.—When you cannot return the inverted womb in the usual way, by pressing chiefly on the neck so as to return first the part which came down last, try continuous elastic pressure. We decided to fall back upon continuous elastic pressure. I therefore sent over from Leeds the necessary instruments—namely, a cup of wood, with slightly-curved stem, the cup being surmounted by a circular india-rubber air-pad, and the stem being set in an india-rubber band, attached before and behind to an abdominal belt. Pressure was kept up by means of this upon the fundus, and attempts at reduction made every day or two, but without any result. I saw the case for the second time on March 6th. For four days previously the pressure had been kept up with great care and very strongly, but no vaginal examination had been made. On introducing the hand, the patient being under chloroform, it appeared at first as if no reduction had been effected, but that the uterus was much smaller. The upper part of the vagina, or what I took to be the vagina, was rather tighter and smoother than the lower and major part of the canal, and this part ended inferiorly at a defined line, but without any ridge or inequality of surface; but it took a moment for the mind to realise that this was really the cervix uteri reinverted, but immensely expanded. The greater part of the uterus was still inverted, but now there was no difficulty in returning it, excepting the last portion, which gave a little trouble. The success of the elastic pressure, the credit of first proposing which is due to the late Dr. Tyler Smith, was in this case complete, and without it I do not believe the uterus could have been reinverted. (Dr. Jas. Braithwaite, p. 315.)

In a case of chronic inversion of the uterus, assisted by Dr. Marshall, I placed the patient on a table on her elbows and knees, drew down the uterus beyond the vulva, introduced a Marion Sims speculum into the vagina, and with a curved bistoury, made three longitudinal incisions into the constricted neck of the tumour, each incision about three quarters of an inch in length and nearly a quarter of an inch in depth; I then passed the uterus back into the vagina, and proceeded to re-invert. The reduction was effected easily, and with a distinct feeling of tearing of the uterine tissue. There was very little hemorrhage during the operation. A morphia suppository was introduced into the rectum, and patient placed in bed. (Dr. W. A. Wilson, Glasgow Medical Journal, 1877, p. 359.)

Irritability of the Actively Secreting Mammary Glands.—With a view to avoid friction, and to secure the full therapeutic effect of belladonna, I had an alcoholic extract prepared of double the strength of the emplas. belladonnæ, but kept fluid by collodion. Camphor was combined with it for the purpose of aiding to arrest the natural mammary secretion. This preparation, now shown, is painted on the breasts much in the same way that you would use blistering fluid. No rubbing in is necessary. The fluid dries quickly, is much more cleanly for the patient, has a less offensive odour than the ointment, and, in my experience, it is more reliable in its action. This liquid preparation is painted over the affected parts of the breast night and morning, until the acute symptoms give in. (Dr. Hugh Miller, Glasgow, p. 322.)

[We find the extract worked up with a little glycerine answer the purpose required admirably, and have employed it thus for many years.—*Eds.*]

Ovariotomy.—I have come to the conclusion that the operation of ovariotomy is beginning to be far too rashly undertaken, or, at any rate, in too early a stage of the disease. Ovariotomy should not be so rapidly and rashly undertaken. I would advise you in every case, to try one tapping at least; it will help in diagnosis, and it *may* cure. I have cured one case in this way which I thought was a growing multilocular cyst. If your patient be comparatively young, and otherwise in fair condition, try a second tapping at least, and carefully ascertain the rate of growth. If, after this, you are satisfied that life is beginning to be jeopardised by the exhaustion of tapping, by wearing pain, or otherwise, then ovariotomy may be performed with hopefulness. I would advise you, in almost every case, to make an incision of fully four to five inches, and not to hesitate to enlarge it at once as freely as seems necessary to command a good view of what you are doing. The treatment of the pedicle is a subject which has from the first been much discussed. You will find in Mr. Wells's work a most temperate and thorough discussion of the various methods that have been used, including its retention outside the peritoneum by the clamp, and its treatment within the peritoneum by ligatures, *écraseur*, acupressure, cautery, &c.; and his vast experience has led him to adopt the clamp for the great majority of his cases. I am bound to say, however, that the use of a strong well-tied silk ligature, cut short and allowed to fall into the abdominal cavity, has so far commended itself to me. The late Dr. Tyler Smith had a very fine series of cases treated in this way; it was this which induced me to try the plan, and it is so superior in

handiness, and in every way, to my mind, so much more simple, that I fancy I am likely to continue its use until I have met with a case where I find inconvenience from the after-presence of the ligature, or until I have been unfortunate enough to meet with one where a ligature of my own tying has proved insufficient to prevent hemorrhage. Very powerful and pure silk, *i.e.*, animal tissue, well carbolised, should be used. This must perforate the stump of the pedicle, so that it cannot slip off at the end, and must then be tied in two separate portions. If the pedicle be very thick or broad, three or even four portions must be tied separately. If these precautions be observed, sufficient force may be used in tying to render any danger from shrinking an impossibility. A touch with the galvano-cautery might add to the apparent safety where the operator is timid; the ordinary cautery is apt to leave behind it some *débris*, which is at any rate unnecessary. (Dr. J. Thorburn, p. 306.)

Intra-peritoneal Ligature of Pedicle.—Dr. Tyler Smith appears to have been the first authority who regularly and systematically advocated complete intra-peritoneal ligature. Recently it has been adopted in hundreds of successful cases where the pedicle has been found too short for the clamp to be safely applied. Ligatures of bleeding vessels in the omentum are also cut short. Mr. Spencer Wells informs me that, on one occasion, he left as many as forty ligatures in the abdominal cavity without any evil effects. In the more perilous operations for the removal of solid growths of the uterus, complete intra-peritoneal ligature may also be practised with impunity; this is proved by Mr. Knowsley Thornton's case, recorded in the "Medical Times and Gazette," April, 1877. That gentleman strongly advocates the use of the silk ligature, even in cases where the clamp has hitherto been thought advisable. An important section of contemporary medical literature furnishes us with a strong proof that complete intra-peritoneal ligature of the ovarian pedicle is firmly established. Mr. Holmes says: "When the clamp cannot be fixed on the pedicle of the tumour, on account of its proximity to the uterus, without injudicious traction on that organ, the best plan is to perforate the pedicle with a needle threaded with stout wire, and tie it in halves, the ends of the ligature having been flattened down so as not to irritate the neighbouring parts, and after cutting away the tumour down to within about half an inch from the ligature, drop the pedicle back into the belly. In a case treated successfully in this way, I searched sometime afterwards carefully for the wire by palpation from the abdominal wall and from the vagina, but could elicit no

sensation of its presence." In 1872 Dr. Bantock exhibited before the Obstetrical Society the stump of an ovarian pedicle from a patient who died of cancer one year after double ovariotomy had been performed upon her. The hempen ligature applied, with its ends cut short, to one of the pedicles, was found on dissection to have been completely absorbed excepting its knot, which remained as a hard body the size of a hemp-seed, covered by peritoneum. Thus the surgeon need no longer dread any evil effects when he thinks it desirable to leave ligatures enclosed in the abdominal cavity after a serious and complicated operation. (Mr. Alban Doran, p. 303.)

Silk Ligature for the Pedicle in Ovariotomy.—I will endeavour to point out certain precautions to be adopted in using the ligature, which will, I think, ensure for it results even more favourable than my own have been up to the present time. First, as to the material. It should be the pure Chinese silk, without any admixture of cotton. The hemp ligature still finds some advocates, but it has, so far as I can see, no advantages, and the great disadvantage of being a vegetable instead of an animal tissue; and it is slippery and cutting to the fingers when wet. One advantage claimed for it is that it shrinks after tying, and hence is a security against hemorrhage. From experiments I have made with it, I do not believe it shrinks at all after it is once thoroughly wet, and this it always is before it is tied; and, if it did so shrink, I should not consider it an advantage in view of what I have already said as to the partial circulation through the constricted portion. The silk, if properly tied, never fails to *sufficiently* constrict. As to the method of applying the silk, I believe it to be important that the two ligatures should interlock when tied, so as to form a figure of eight, otherwise they may pull apart at the point of puncture and hemorrhage may result. In a thin pedicle, it is easy to avoid puncturing a vessel, and the interlocking of the ligatures is a matter of less consequence; but, in a thick one, it is often impossible to see and avoid every vessel of sufficient size to be dangerous if punctured. If more than one transfixion be required, the ligatures should all interlock so as to form a chain. This has one disadvantage, tending, in a broad short pedicle, to cause a drag upon the outer loop of the ligature-chain, and hence to increase the risk of hemorrhage from slipping of some of the large veins. The weight of the uterus and other ovary, aided perhaps by the contractile nature of the tissue of the broad ligament, increases this risk. It is well, therefore, in every case, to examine the pedicle-stump carefully just before closing the incision; and, if any sign of hemorrhage or

slipping be found, to apply a fine silk or catgut ligature by transfixion on the proximal side of the other ligatures, this last ligature to include the veins already referred to. I always adopt this plan now, using either fine silk or catgut wherever the outer edge of the broad ligament feels tight as one passes the fingers along it from the pelvic brim to the edge of the pedicle. In all the earlier cases, I employed very thick silk, and seldom more than a single transfixion, with an accessory ligature if necessary. Thinking this thick silk might be the chief cause of the trouble with the pedicle, I tried a medium thickness, tying the pedicle in smaller pieces and with more transfixions. I found the results satisfactory, and I now use the finest silk that will bear the necessary strain in tying. (Dr. J. K. Thornton, p. 297.)

Pessaries.—The evidence as to the value of Hodge's pessary, in all its modifications, is overwhelming, and it is now too late to take refuge in systematic pooh-poohing. And while it is capable of doing much good in skilful hands, it must not be forgotten that it is equally capable of much injury in the hands of the ignorant or careless. I am afraid it is a fact that many men pass through our medical schools, destined for general practice, without ever having seen a pessary applied. This I say as a result of experience. There are other instruments besides Hodge's pessary of great service in uterine displacements, notably Zwanke's pessary. The flat ovoid boxwood pessaries are, I would fain hope, things of the past. Zwanke's pessary is, however, an instrument of great capabilities in cases of prolapsus in the young, and affords great comfort to the aged. The facility with which it can be applied and removed by the patient is a great recommendation. The habit of employing hard vulcanite pessaries is apt to contribute to this misuse; they are not kept in sufficient variety, and are difficult to alter. I have now for some years been in the habit of using those made of pewter. They are obtainable from Krohne and Sesemann in nine sizes. They are light, easily altered, and produce no irritation. I have known these instruments worn continuously for over a year without undergoing any change; and they have this advantage, that the presence of excoriation or ulceration is at once revealed by the blackening of the metal, which can be seen without removing the instrument. (Dr. Geo. G. Bantock, p. 269.)

I have given up the use of intrauterine pessaries. The support, by a Hodge or similar vaginal pessary, of a large tender displaced uterus I esteem a plan of treatment well worthy of trial, and act accordingly. (Dr. J. Matthews Duncan, Lancet, Feb. 2, p. 181.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art 1.—ON LACTIC FERMENTATION AND ITS BEARINGS UPON PATHOLOGY.

By Joseph Lister, Esq., F.R.S., Professor of Clinical Surgery in King's College, London.

Prof. Lister (in a paper read before the Pathological Society of London) said that a few years ago it would have been deemed very improbable that the souring of milk should have any bearing upon pathology, but the large and influential audiences which Dr. Sanderson's lectures, at the University of London, were receiving, were sufficient evidence that the essential nature of fermentative changes is occupying a foremost place in the minds of pathologists. In reading reports on the discussions which had taken place upon diseases of such a nature—for example, that of pyæmia, at the Clinical Society—it had seemed to him that medical men were apt to begin the inquiry of these subjects at the wrong end; to view them rather from the clinical than the pathological side. Then it seemed to him that, before any sure steps could be taken with regard to the nature of fermentative diseases in the human subject, it was necessary to have clear ideas, distinct opinions, and positive knowledge, on those fermentative processes which are the more simple, because they can be observed in our laboratories. It might be said that sufficient had been done in this direction by Pasteur and others; but this was not universally accepted, for there are men ranking high as physiologists and pathologists who take exception to the conclusions drawn by Pasteur on the ground that the organisms found in association with fermentative changes, such as putrefaction, are only accidental concomitants of the process, and not essential to it. Some time ago he had made an attempt in the special case of lactic fermentation to satisfy himself one way or the other as to the nature of the change. He succeeded in so satisfying himself, and it occurred to him that it might not be without interest to the members of the Pathological Society to see the preparations which led him to his conclusions before, from lapse of time, they should have lost their value. Sill he should not have presumed to bring the subject before the

Society had he not also some further points to add beyond those he had already published. First, as regard the method of experimentation, he had found that if an organic liquid, whatever it be, free from organisms, be placed in a perfectly pure glass (i.e., pure of anything living) which is covered by a glass c ap also pure, that with a glass shade, and the whole placed on a plate of glass, no organisms would occur in the liquid. Or, in other words, although the atmosphere has free access to the liquid, for the different coverings do not fit closely, yet the double protection is sufficient to exclude the atmospheric dust; and if the dust is excluded, organisms are prevented from occurring. The glasses are obtained pure by submitting them to a temperature of 300° F. for two hours. As far as his experience went, that heat was sufficient to destroy the life of all living material. But, in addition to this, the air gaining access to the box in which the glasses were heated should be filtered from dust, and the filtration was effected by having the door of the box packed round with cotton wool, which serves as an effectual filter. In order to heat the box equally throughout, a shelf was placed between the Bunsen's burner beneath and the bottom of the box, and at the same time a cover was put around the box, with a hole at the top to allow the escape of hot air and the entrance of a thermometer surrounded by cotton-wool into the box, and the glasses to be purified being placed in it, the vessel was heated to 300° F. for two hours, and when cold the glasses taken out, and known to be free from living organisms. The next step was to introduce the organic liquid in a pure state into such a glass. Mr. Lister said that he had previously described a very complicated method of effecting this; but he had now followed a comparatively simple plan. He used a flask provided with a second neck projecting from the side, and bent at an angle near its extremity. The commencement of the neck was large, and the terminal part short and narrow. With such a flask, the end of the nozzle is always valved by a drop of the fluid after passing, so that regurgitation of air through the nozzle can never take place. At the same time the mouth of the flask is covered with carbolised cotton wool (conveniently prepared by steeping the wool in 1 per cent. solution of carbolic acid in anhydrous ether), and a cap of such wool is also placed upon the nozzle after the drop at its end has been removed by the application of a carbolised rag; so protected, the liquid will remain pure in the flask to be used again a week, a month, or a year hence. (Mr. Lister showed a flask of Pasteur's solution, prepared on Aug. 7th, which remained perfectly clear.) Then, in decanting, and in order to protect the glass to be charged from contamination during the process, he substituted for the

cotton-wool cap a hemispherical shield formed of half an india-rubber ball perforated to allow of the passage of the nozzle. The india-rubber, previously soaked in carbolic acid lotion, which it readily absorbs, was rendered antiseptic, and the hemispherical form of the cap prevented the collection of dust. In that way many glasses could be charged which would remain with their contents unaltered till they dried up from evaporation. The flask in which the organic liquid was stored was first purified by heating in the hot box; and the question then was how to introduce into it a pure liquid, uncontaminated by atmospheric dust. This could easily be done in the case of unboiled urine, for if the urethra and bladder be healthy, all that had to be done was to apply a solution of carbolic acid (1 in 40) to the glans penis and the meatus, and the urine passed directly into the flask. It would remain with its vesical mucus for any length of time without any bacteria arising in it. It was, however, impossible to get milk pure to start with; it was necessary to purify it with heat. He did not know what heat would be necessary for this purification in the densely-populated metropolis he had come to, but in the comparatively pure metropolis he had lately inhabited he had never found any of the organisms present in milk to resist a temperature of 210° F. for half an hour—*i.e.*, in the moist state. His plan was to introduce the flask containing the liquid into a saucepan of boiling water, and in consequence of a certain amount of evaporation, the temperature in the flask was somewhat short of 212°. This was an advantage, for no frothing occurred in the fluid from its ebullition. It was necessary also to introduce the liquid into the *lower* part of the flask, taking the greatest care that not a single drop should come in contact with the upper part, which would not be heated to the full heat of the saucepan. Using a funnel previously purified by a 1 in 20 solution of carbolic acid, and passing this through a wrapper of cotton-wool in the mouth of the flask, the liquid could be poured into the latter, precaution being taken on the withdrawal of the funnel that it did not touch the sides of the flask, which was then immersed in the boiling water. In this way he had filled the flask with Pasteur's infusion, turnip-water, urine, &c., and always succeeded in preventing the development of organisms. But with milk the same proceeding constantly failed. Some might have said that the reason of this failure depended upon the existence in milk of complex substances ready to develop into organisms, and which resisted the influences to which they had been subjected, the more simple materials in Pasteur's solution being more readily acted on. But, for his part, he felt sure the failure was due to some defect in the methods employed, and he soon found an explanation

in the fact that in pouring fluid through a funnel it carries air-bubbles with it. Now milk is a pabulum for all kinds of organisms; nearly all varieties of bacteria (and there are indeed very many varieties) will live in milk; whereas only a small proportion of such organisms will live in Pasteur's solution. To obviate the introduction of air containing bacteria, he substituted a syphon for the funnel; the syphon being composed partly of glass and partly of india-rubber, upon which a clip was placed. The syphon being previously filled with water higher than the temperature of the air (so that no bubbles could be formed in it during the process), it was then easy to introduce the liquid to the bottom of the flask without introducing the smallest bubble of air. Again, in withdrawing the syphon, care must be taken that it does not touch the edge of the flask. Since he had adopted this method, he had charged many flasks with milk which had been heated to 210°, and had never failed in keeping them free from organisms. He showed a flask which had been so charged on August 7th, and the contents of which remained as pure now as on that day. Mr. Lister remarked that this failure and its correction were instructive, as showing that the development of organisms under circumstances in which they might not be expected to develop was liable to be explained by fault on our part—defects in our manipulation. Having thus described at length the method of experimentation, Mr. Lister proceeded to say that he had selected lactic fermentation, first, because of its remarkable character, peculiar in the solidification it gave rise to, and in the marked souring of the milk accompanying it; and secondly, because the ferment causing it is a *rare* ferment, and if it be a rare ferment, it was not likely that any accidental defect in the manipulations would cause its introduction. It was rare in the sense that it was present only in dairies; there it seemed to be universal, but in the world in general it was rare. Thus, if specimens of boiled milk be exposed in different places—anywhere, in fact, except in a dairy—fermentative changes would be set up, organisms would develop; but it would certainly not be the lactic fermentation, nor would the organism peculiar to it—bacterium lactis—be found. This bacterium is motionless, occurs mostly in pairs or in leptothrix chains, or in groups of three or four, and is invariably present in souring milk. But it does not appear as the result of mere exposure to the air. He showed several test-tubes, containing milk which had been received into a pure glass direct from the cow, at a short distance from a dairy. Every one of them had undergone fermentative changes, and in some of them simple examination with a lens showed the presence of filamentuous fungi, apparently of five different species; so that in spite of the care

taken, organisms had entered, but none of them had undergone lactic fermentation. They contained bacteria and fungi of all sorts, some as scarlet spots, others as golden-yellow, but not one of them contained the bacterium lactis. Mr. Lister showed also another set of glasses containing milk taken with greater precautions, in which it might be supposed that no change had occurred, but on microscopical examination some were found to contain bacteria; one or two remained free, showing that unboiled milk has no ferment in it tending to organic development. He had found also that by adding drops of water to series of glasses of boiled milk fermentative changes may be set up, but not the lactic fermentation. By means of a graduated syringe he could introduce so small a quantity as one-hundredth of a minim of water into a glass of boiled milk; and he found that, taking ten glasses, in some of them organisms appeared, and fermentative changes took place, but others escaped these changes altogether. Moreover, different fermentations occurred in different glasses. This proved the fact pointed out by Dr. Sanderson that the fermentative agency in water is not in a state of solution, but in the form of suspended particles. For were it in solution then there would be no reason why some of the milk should escape altogether, and some again be the seat of changes different from what took place in others. But he would again insist that, various as these fermentations were, no instance of lactic fermentation occurred. This then pointed to the fact that the souring of milk could not depend on anything inherent in the milk itself, but something introduced from without, and that not present in water or in the air in general, but only in dairies. But even in a dairy he once exposed a specimen of boiled milk for fifteen minutes, and the result was a growth of filamentous fungi, and a peculiar bacterium, which was associated with a curious viscidity which he could only liken to that of a spider's web, so that he was able to draw a thread of the altered fluid up by a needle for a distance of a yard and two inches before it broke; but the bacterium lactis did not occur; and this was the only occasion on which he had not found it develop in milk exposed in the air of a dairy. Before proceeding further Mr. Lister said that he wished to make a confession. Next to the promulgation of new truth the best thing a man can do is the recantation of published error. Some years ago he published in the *Microscopical Journal* a description of the behaviour of bacterium lactis in different liquids. Thus inoculating a specimen of urine with sour milk, he found developed a small, long, motionless spirillar bacterium, and a second inoculation from the first produced similar appearances. From the product of this second inoculation he inoculated some

Pasteur's solution, and got active bacteria as a result. Then by a reverse process he reproduced the spirillar form in urine, and inoculating milk with this he reproduced lactic fermentation. He also thought he could distinctly trace transitions between all these forms, which he believed to be bacterium lactis modified according to its environment. Having lately adduced these as facts in an argument with an eminent physiologist, who, on hearing them, said they carried conviction to his mind, Mr. Lister thought that he would repeat the experiments, seeing how valuable a support they rendered to his views. But this time he got quite a different result, for in the inoculated urine, instead of the coiled motionless organism, he got an active double bacterium, whilst in Pasteur's solution he got a motionless bacterium. How could these irregular and discordant facts be explained? Obviously on the ground of some accidental contamination; and he was determined to get rid of accidental concomitants which would lead to the development of bacteria other than bacterium lactis. To do this he diluted the milk with so much boiled water that there should only be on an average a single bacterium to every drop of the inoculating fluid. Then, as in souring milk the bacterium lactis is in enormously larger proportion than other forms, the chances wonld be that the drop would only contain bacterium lactis. He therefore counted the number of bacteria (which were active as well as motionless) in a specimen of souring milk, and he found that the amount of dilution necessary for the purpose above mentioned was 1,000,000 parts of water. This dilution effected, and urine inoculated with a drop of the diluted milk, he got developed in the urine the bacterium lactis; but so small in size, and so scarce in quantity, that, if any other bacterial forms had been present, the latter alone would have been seen. Then inoculating a specimen of Pasteur's solution from the urine containing the bacterium lactis (and, to ensure the result, using a "separation tube," so constructed with a plug of wool in its interior as to allow only moving organisms to pass through it), he could not obtain any result. Repeating this experiment by direct inoculation with the diluted sour milk, it was found that no development ensued in the liquid, but after a long time the morsel of curd introduced was replaced by a mass of minute bacterium lactis, showing that while the Pasteur's solution did not prevent the development of the organism, it afforded no pabulum for its support. Therefore, it seemed that he was in error in the inference he drew from his former experiments. Excluding the chances of accidental contamination, he failed to get the results he then did. Knowing, then, how extremely numerous these organisms are, we can understand how such fermentations

arise; and ascertaining how many bacteria there are in one-hundredth of a minim of milk, we can find how much water should be added, so that one drop should contain one bacterium. But it was hardly likely the bacteria should be uniformly distributed for inoculating ten glasses of boiled milk by such diluted drops. Five of them had been affected with lactic fermentation, and five had remained fluid. A drop of the fluid from one of these was under the microscope. No bacteria could be seen in it, only the milk globules were becoming angular from drying. Mr. Lister showed a glass containing milk which had undergone simply lactic fermentation. The curd was of a pure white, and the milk had a sour odour. But in others, which had been allowed to "sour" in the ordinary way, numbers of other organisms were present, producing other fermentations, such as the butyric, that produced by the oidium lactis, &c., and finally the putefractive. It had been shown that in water the fermentative agency occurs in the form of suspended particles, and not in solution; and the experiment just recorded proved the same with regard to the lactic fermentation. For, supposing even, for the sake of argument, one admitted the most unwarrantable assumption that each drop contained chemical particles capable of self-multiplication as rapidly as bacteria (and a bacterium may be watched to double itself in an hour), it would be quite inconceivable—the bacteria then being mere accidental concomitants—not only that the inorganic particles and the organised should be present in precisely the same numbers, but that they should always go in pairs, so that every drop which started the lactic fermentation should also contain a bacterium. For in every glass in which the fermentation occurred the bacteria were found. This demonstrated an important point, which may be applied to other instances. The sour smell accompanying the pure lactic fermentation was an interesting subject of inquiry. It could not be due to lactic acid, because that is absolutely non-volatile; and it was different from the odour of butyric acid which prevailed in ordinary souring of milk. Mr. Lister had distilled some milk which had undergone the pure lactic fermentation, and found the distillate to have still a pungent odour, but it did not have an acid taste nor affect litmus paper. Hence he concluded the sour odour must be due to some form of ether or allied substance the nature of which he must leave to the chemists to determine. Mr. Lister then called attention to a diagram upon which were drawn to scale various forms of bacteria, and he drew attention to the fact that the bacterium lactis developed after dilution with 1200 parts of water was of extremely small size, the particles approaching the minimum visible, their grouping alone deter-

mining their nature. Compared with them the torula cerevisiæ appeared large; in fact the bacterium lactis was not so large as the granules in the torula. Was it not conceivable that there might exist other organisms as much smaller than the bacterium lactis as that was than the torula? and, admitting the probability of this, we could conceive of the existence of ultra-microscopical organisms. Therefore he held that because we do not find any organisms in many morbid conditions—e.g., erysipelas (which he had repeatedly examined to this end)—yet the existence of such may be as real, and they may produce as potent effects, although we may not be able to see them with any microscope that we have, or be likely to see them with any microscope that will be produced. He had found that in three days the bacterim lactis diminishes much in size, so that to see it in its best and largest condition it was necessary to inoculate a drop of boiled milk by dipping into it the point of a needle just dipped into sour milk, and then examine the drop. In twenty-four hours the bacteria would get smaller, and at last the majority would be so small, and so entangled in the curd, like blood-corpuscles in coagulating fibrin, that they may not be seen. Some years ago he found that after keeping some tap-water in a pure flask for forty-eight hours he could detect a bluish film on the surface, composed of bacteria of extreme minuteness, and he could not doubt that they had existed in the running water, but from their size had been invisible. It must be remembered also that in the experiments he had related the organism produced the fermentation in the hundredth of a minim—that is to say, in a drop which would cover a surface of half a square inch. One might travel for a whole summer day over such a field and fail to find a bacterium, even if it were of large size, but when to this is added the extreme minuteness, who would venture to say that because they could not *see* the bacteria they must be absent? He had never had evidence of a microscopical germ; but he was aware that only in two forms, one of which was bacillus anthracis, had evidence of such spores been adduced. Indeed it was very probable that bacteria do not require a germ. They are, as it were, reproductive organs in themselves, and if there be any organisms in existence which do not require a germ, bacteria are such organisms. Thus, concluded Mr. Lister, what appears to be a somewhat needless mystery may be capable of a very simple explanation.

[In the discussion which followed, Dr. Burdon Sanderson expressed the extreme pleasure he felt in listening to the beautiful exposition of the facts of fermentation, and his concurrence in the conclusions drawn by Prof. Lister.]—*Lancet, Dec.* 22, 1878, *p.* 918.

2.—THE BEARING OF EXPERIMENTAL EVIDENCE UPON THE GERM-THEORY OF DISEASE.

By Dr. H. Charlton Bastian, F.R.S., F.L.S., Professor of Pathological Anatomy in University College, London.

Though it may be conceded that with our present state of knowledge an affirmative decision in regard to the absolute proof of the present occurrence of archebiosis may be still withheld, there is, I think, no similar warrant for suspense of judgment in regard to the germ-theory of disease, or, as it is also called, the doctrine of *contagium vivum*. Existing evidence seems to me abundantly sufficient for the rejection of this doctrine as untrue.*

My urine and potash experiments will go far to illustrate this difference in the weight of the evidence in regard to the two questions.

A "sterilised" fluid—that is, one which, left to itself, would always remain pure—may be caused to ferment by the addition of a certain proportion of liquor potassæ devoid of all living things, especially if the influence of the potash be favoured by certain accessory physical conditions. This fact is admitted by M. Pasteur himself. During the fermentation thus initiated, a matter (ferment) appears and increases, which is capable of spreading a similar process far and wide in suitable media.

But, on the strength of the analogy upon which the germ-theorists rely, we may find in such an experiment a warrant for the belief that in a healthy person, free from the contagium of typhoid fever or any other of its class, certain kinds of ingesta (solids or fluids), wholly free from all specific poison, may, with or without the favouring influence of other altered conditions, give rise to an independent zymotic process. And during the process thus initiated, a matter (contagium) appears and increases in certain of the fluids or tissues of the body, which is capable of spreading a similar disease far and wide amongst receptive members of the community.

* Since this paper was read, the doctrine has again been proclaimed—and never with more force and ability—by Dr. William Roberts (*Brit. Med. Journal*, Aug. 11, 1877). Its essential points may be stated in the words of its latest exponent. He says: "I have already directed your attention to the analogy between the action of an organised ferment and a contagious fever. The analogy is probably real, in so far, at least, that it leads us to the inference that contagium, like a ferment, is something that is alive. . . If, then, the doctrine of a *contagium vivum* be true, we are almost forced to the conclusion that contagium consists (at least in the immense majority of cases) of an independent organism or parasite; and it is in this sense alone that I shall consider the doctrine, . . . it is more than probable, looking to the general analogy between them, that all infective diseases conform in some fashion to one fundamental type. If septic bacteria are the cause of septicæmia, if the spirilla are the cause of relapsing fever, if the *Bacillus anthracis* be the cause of splenic fever, the inference is almost irresistible that other analogous organisms are the cause of other infective inflammations and other specific fevers."—Sept. 1877.

Can the germless liquor potassæ, *plus* the favouring conditions (the principal of which is a certain high temperature), be regarded as the "cause" of the fermentation? The answer does not admit of doubt; the effect in question would not have taken place without their influence. The old logical formula in regard to the word, *cessante causâ cessat et effectus*, completely justifies this point of view, and so also does the definition of Sir John Herschel. A "cause," said this philosopher, is "an assemblage of phenomena which occurring, *some other* phenomenon invariably commences or has its origin."

But there is a point of view which must not be lost sight of. It is of considerable importance, and has of late been dwelt upon by G. H. Lewes with his usual force and clearness. He says: "The fact, that it is a convenience to select some one element out of the group, either for its conspicuousness, its novelty, or its interest, and that we call it the cause of the change, throwing all the other elements into the background of *conditions*, must not make us overlook the fact that this cause—this selected condition—is only effective in coalescence with the others. Every condition is causal; the effect is but the sum of the conditions."

This brings us to the only point of doubt which can possibly exist in regard to the interpretation of my experiment. It is, whether our most prominent causal element, the liquor potassæ, exercises its influence (*a*) partly upon the fluid and partly upon certain otherwise dead or impotent germs still lurking within the vessel, or (*b*) simply upon the mere chemical constituents of the fluid medium, but in such a way as actually to engender minute particles of living matter which thereafter appear as ferment-organisms.

If a practically dead germ can by any treatment be revived, it may take its place as one of the causal conditions leading to fermentation; hence it is that a certain reserve may still be maintained as regards the absolute proof of the possibility of a germless origin of common fermentations, and the almost simultaneous occurrence of a new birth of living units (Archebiosis).

But all similar grounds for reserve are absent—are non-existent, in fact—in regard to the bearing of this experiment upon the possibility of an occasional independent origin for zymotic disease, whether or not such disease is characterised by the appearance within the body of any distinctive living organisms.

This I will now endeavour to demonstrate.

It is the process of fermentation which is supposed to be in part analogous to the zymotic disease. It is true that a contagious something becomes engendered during fermentation and

during zymosis, by means of which the process or the disease may be spread abroad. But there are important differences in regard to the possible independent origin of the two processes which have hitherto been only too much neglected. The treatment of this subject has often been much too superficial. In order to produce a kind of pictorial effect which may easily captivate the imagination, difficulties are often ignored, and many new, modifying, or antagonistic points of view have even of late been treated as though they were non-existent.

A few words will suffice to make plain some of the differences between the respective conditions which would be operative in the germless origin of fermentation on the one hand, and in the *de novo* origin of a contagious disease on the other. And in so doing, I shall be able, I think, at the same time, to show how much simpler it would be to bring about an independent zymosis than an independent fermentation—that is, if we are to rely on the analogy upon which the germ-theorists base their arguments.

During the great majority of fermentations, living organisms make their appearance and rapidly multiply. These living organisms have been proved to be common producers of chemical principles, some of which are soluble ferments, others (like pyrogen) are poisons which may be almost as deadly as that of a serpent, whilst others still are inert and appear as mere pigment granules. It is proved that some of these chemical principles act as true ferments. It is thought, and it is probable, that the organisms themselves—altogether apart from their media and what else they may contain—may be capable of doing the same. Still this has not yet been definitely proved; so that the action of soluble chemical ferments is at present almost better substantiated than that of the living organisms by which they may have been formed. By means of boiling alcohol and other agents, these bodies can be isolated and freed from living impurity. It is, however, much more difficult entirely to separate minute living organisms from their media, and consequently more difficult to be perfectly certain in regard to their potencies. It is, however, on account of the derivation of the chemical ferments from the living units, and because of the presence of these latter bodies in all fermenting mixtures, that their own agency is still regarded by many as essential to the initiation of ordinary fermentations. But, as I have already indicated, we much need further information as to the precise mode in which fermentation is initiated and carried on by soluble ferments like that which M. Musculus discovered in and separated from urine. If they (all or any of them) are capable of setting up fermentations in germless fluids, in the course of which

organisms appear, such phenomena would most effectually disprove an exclusive germ-theory.

Turning now to the process of zymosis, we find the available generative conditions altogether different. Here we have to do not with fluids only, but with tissues and organs composed of living elements characterised by all kinds and degrees of activity. Some of them produce the varions soluble ferments of the body, some may produce poisons, and others habitually lead to the formation of pigment-granules—vital acts severally similar in kind to those which the common ferment-organisms are known to manifest. Tissue elements without number having such and multitudes of other properties are therefore ever present, capable, under certain influences, of being more or less easily diverted into unhealthy modes of action, so that many of them may become true living ferments in the modern sense of that term, and, therefore, possible producers of chemical ferments (contagia) capable of initiating some or the whole of the series of changes by which they were themselves produced, in other suitable sites.

The essential difference between the two problems thus becomes plain. The only point which my experiment leaves in the least doubtful in regard to the causal conditions initiating fermentation is, whether any latent, powerless, and, as it were, dead organised ferment may still, in spite of the usual evidence to the contrary, lurk in the seemingly "sterilised" fluid. This, however, is the very point about which there is no shadow of doubt in regard to zymosis. Possible ferments without number are, by necessity, present in the form of tissue-elements. So that if we are to be guided by the analogy upon which all germ-theorists so strongly rely, the independent generation of a zymotic process should, for the reason above specified, be incomparably more easy to be brought about than fermentation in a germless fluid. In regard to the independent origin of a zymosis, the all-important point is, not whether latent ferments exist, but whether any causes, or sets of unhygienic conditions, can rouse or modify, in certain special modes, the activity of any of these myriads of potential ferments of which the human organism is so largely composed. And if, as some germ-theorists would have us believe, impotent germs of common ferment-organisms, incapable of exclusion, are also widely disseminated throughout the body, these, if they are such unavoidable elements, could (in regard to the etiology of disease) only be looked upon as components of the body, ranking side by side with the tissue-elements themselves.

Thus such organised ferments or germs as are possibly absent from the "sterilised" experimental fluids are confessedly present by myriads in persons who may be sickening under the

influence of various unhygienic conditions or non-specific states of the system; and the only point which is regarded as doubtful in connection with the *de novo* origin of a zymosis, is what analogy might lead us to affirm as completely proved by my experiments, viz., that certain conditions, or states of system, may be capable of rousing some of such ferments into a specific kind of activity, wholly apart from the influence of any specific contagia coming from without.

Even if independent ferment-organisms of common or special kinds do make their appearance during any process of zymosis originated in the manner above suggested, they would, from the point of view of the etiology of disease, be just as much consequences of the morbific influences, as proliferation of tissue-elements is a consequence of the direct application of acetic acid or any other irritant.

But here, in order to make this point of view more plain, a short digression is necessary.

The intracellular fermentation in vegetal tissues supplies us with a kind of link between the ordinary processes of fermentation and the zymotic processes of animals. MM. Lechartier and Bellamy, as well as Pasteur and others, have now clearly shown that in vegetal tissues placed under certain abnormal or unhealthy conditions, fermentative phenomena take place essentially similar to those occurring in solutions containing independent ferment-organisms. And just as the vegetal cell can do what, in other cases, the independent organism does, so it is supposed that in the process of zymosis, tissue-elements may take on a specifically faulty action, leading to the formation of certain chemical principles or "contagia" in the fluids or tissues of the animal body; so that, in the great majority of zymotic diseases, offcast particles from the body, whether living or dead, when saturated with such principles, may constitute the veritable contagia by which the specific disease is spread abroad amongst the community.

In the majority of the cases of intracellular fermentation, no independent organisms are generated, though in others, as in that of the beetroot and the potato, they are invariably concomitants. Similarly, in the majority of zymotic diseases, no independent organisms are generated, though in others, such as relapsing fever and splenic fever, they are invariable concomitants; and being engendered in diseased parts and fluids, they may thereafter themselves act either as real contagia or as carriers of contagion.

The causal conditions capable of inducing fermentation in the beetroot and the potato, and with it the appearance of bacteria in swarms throughout their tissues, are known, and have no ordinary connection with pre-existing bacteria. And

similarly, the causal conditions capable of inducing relapsing fever and splenic fever, though not so definitely known, may nevertheless have no ordinary connection with pre-existing sprilla and bacilli resembling those which appear in the blood or tissues of the patients suffering from either of these diseases.

Thus the mere fact, that in certain zymotic diseases living organisms have been proved to appear, affords of itself no support whatever to an exclusive germ-theory, as I shall, after this digression, endeavour to show.

The fact may be quite otherwise explained, either (1) in accordance with the views of certain germ-theorists, though these are in direct opposition to the statements of others of the same party; (2) in accordance with the statements of the second section of the germ-theorists, supplemented by a belief in heterogenesis.

(1) The presence of latent germs of common, though modifiable, ferment-organisms throughout the body is invoked by one section of the germ-theorists, who contend that certain altered states of health, together with altered vitality of tissues, may rouse such hitherto latent common organisms into activity, and occasionally convert them into so-called "specific" forms capable of new actions. But based as this view is upon wholly insufficient evidence, and with its fundamental position denied by other leading germ-theorists, it would, even had it been securely founded, be quite inadequate to meet the necessities of their position. A special zymotic disease, which had arisen in the manner above indicated, would assuredly have had what is termed a *de novo* origin—it would have started from no specific cause, and would never have developed, but for the existence of those "determining conditions" which brought about the altered state of health and tissues. This group of conditions would therefore constitute the cause of the disease; and inasmuch as, by the hypothesis we are now considering, the common germs are held to be *ever present and unavoidable*, any changes or developments which they might take on could only be studied in the same rank and side by side with those of the other tissue-elements, that is, as consequences or phenomena of the disease.

(2) It was originally affirmed by Professor Burdon Sanderson, and it has of late been distinctly reasserted by M. Pasteur, that the blood and internal tissues of healthy animals and of man are entirely free from ferment-organisms or their germs. Some have sought to modify this view, on the strength of certain experiments, which are so extremely inconclusive as to make it almost puerile to have brought them forward.

For, however strong the evidence is that living units may, on

certain occasions, be even proved experimentally to appear in fluids, in which no living matter previously existed (archebiosis), it is even stronger to show that, under certain conditions, similar low independent forms of life may originate in the midst of living tissues previously free from them, by a kind of transformation (heterogenesis) of some of the units of protoplasm, which, though still living, have been modified in nature and tendency by reason of their existence in a partially devitalised area.

The evidence in favour of this last kind of change may be regarded wholly apart from that furnished by the closed flask experiments, from which it is quite distinct. It suffices, I think, to account for the presence of organisms in some of those local and general diseases with which they are known to be associated, and therefore to complete the proof that even such disease may originate *de novo* (as well as by contagion), and that the organisms which characterise them are, in such cases, consequences or concomitant products, not causes, of the local or general conditions at whose bidding they appear. The elements of the proof are these:

(*a*) First, there is the evidence which has been adduced by various observers as a result of the study by the micoscrope of the mode in which organisms appear within tissue-elements. I do not lay much stress upon this here, because evidence of such a nature is more open to various objections than that which is to follow.

(*b*) Although the blood and internal tissues of healthy animals and of man are free from independent organisms and their germs, yet such organisms will habitually show themselves after death, in the course of a few days, throughout all the organs of one of the lower animals or of man—even when life has been abruptly terminated during a state of health. It cannot be said, in explanation of this, that the organisms naturally present in the intestinal canal have been enabled to spread through the body so as to reach its inmost recesses *after death*—since many of the organisms found are motionless, and others have mere to-and-fro movements of a non-progressive character. The blood, again, has ceased to circulate, so that this fluid, germless during life, cannot after death be considered to act even as a carrier. If the organisms themselves cannot make their way through the tissues, and if no carrier exist, they must naturally have been born in or near the sites in which they are found.

Phenomena of this kind are to be witnessed even in insects, such as silkworms and flies; and the organisms that habitually develop in them after death are, as in the case of higher animals, just such organisms as appear in some of their best-

known contagious diseases. Certain of these diseases, like "muscardine," seem to be generable *de novo* at the will of the operator, by merely placing the animal for a few days under particular sets of unhealthy conditions.

(*c*) Some of the ferment-organisms may also be made to appear at will in certain parts of still living and previously healthy animals by determining in any such part either (1) a greatly lowered vital activity, or (2) an active perversion of the nutritive life of the part of considerable intensity.

1. This subject has been studied experimentally by Messrs. Lewis and Cunningham, two thoroughly competent and trustworthy observers, whose researches during recent years have won for them a deservedly high reputation. They say: "The object of the experiments was to ascertain whether, by interfering with the vascular supply of certain tissues and organs of the body of an animal without injuring the isolated tissue, we should be able, within the course of some hours, to detect organisms in those parts in the same manner as we had been able to do when an animal had been killed under chloroform and set aside in a warm place. We found that such was the result, and that a kidney, for example, when [its artery was] carefully ligatured without interfering with its position in the abdomen, would be found after some hours to contain precisely similar organisms; whereas the other kidney, whose circulation had not been interfered with, contained no trace of any vegetation whatever."

2. Facts of this second order have been thoroughly established by the important researches of Professor Burdon Sanderson. He says: "If a few drops of previously boiled and cooled dilute solution of ammonia are injected underneath the skin of a guinea-pig, a diffuse inflammation is produced, the exudation liquid of which is found after twenty-four hours to be charged with bacteria." "Other chemical agents," he adds, "will lead to the same results, and always under conditions which preclude the possibility of the introduction of any infecting matter from without."

Elsewhere the same investigator refers to experiments which were made about the same time in order to throw light upon the cause of the appearance of bacteria in certain peritoneal exudations, and to ascertain whether or not their presence was to be considered as "a mere result of the intensity of the peritonitis." He says: "To determine this, experiments were made during the following month (May, 1871), which consisted in inducing intense peritonitis by the injection, not of exudation liquids, but of chemical irritants, particularly dilute ammonia and concentrated solution of iodine in hydriodic acid. As regards the ammonia, precautions were taken to guard

against contamination by boiling and cooling the liquids, as well as the implements to be used, immediately before injection. In the case of the iodine solution, this was, of course, unnecessary. In every instance it was found that the exudation liquids, collected from twenty-four to forty-eight hours after injection, were charged with bacteria, whence it appeared probable that the existence of these organisms was dependent, not on the nature of the exciting liquid by which the inflammation was induced, but on the intensity of the inflammation itself."

From the various evidence, more or less fully referred to in the present section, it seems to me legitimate to conclude:—

First, that if we are to be guided by the analogy now dwelt upon as existing between fermentation and zymosis, it would be perfectly certain that the latter process can originate *de novo* —that is, under the influence of certain general or special conditions, and where specific contagia of any kind are at first absent, though they subsequently appear as results or concomitant products. So that an exclusive theory of "contagion" as the only present cause of communicable diseases, is not supported by experimental evidence.

Secondly, that some contagia are mere not-living chemical principles, though others may be living units.

Thirdly, that even in the latter case, if the primary contagious action be really due to the living units and not to the media in which they are found, such primary action is probably dependent rather upon the chemical changes or "contact actions" which they are capable of setting up than upon their mere growth and vegetative multiplication.

Fourthly, that, where we have to do with a true living contagium (whether pus-corpuscle or ferment-organism), the primary changes which it incites are probably of a nature to engender (either in the fluids or from the tissue-elements of the part) bodies similar to itself, so that the infected part speedily swarms therewith. When pus from a certain focus of inflammation comes into contact with a healthy conjunctiva, and therein excites a contagious form of inflammation, no one adopts the absurd notion that all the pus-corpuscles in this second inflammatory focus are the lineal descendants of those which acted as the contagium; and the mode of action may be altogether similar when matter containing bacilli, by coming into contact with a wounded surface, gives rise to splenic fever and the appearance of such organisms all through the body. The old notion about the excessive self-multiplication of the original contagium is probably altogether erroneous.

Thus, all the distinctive positions of those who advocate a belief in the so-called "germ-theory of disease," or rely upon

the exclusive doctrine of a "contagium vivum," seem to be absolutely broken down and refuted. We may give that attention to the appearance and development of independent organisms in association with morbid processes which the importance of their presence demands, but we must regard them as concomitant products, and not at all, or except to an extremely limited extent, as causes of those local and general diseases with which they are inseparably linked."—*British Medical Journal, Jan.* 12, 1878, *p.* 49.

3.—THE BEARING OF EXPERIMENTAL EVIDENCE UPON THE GERM-THEORY OF DISEASE.

By Dr. Maclagan, Physician to the Dundee Royal Infirmary; Examiner in Medicine to the University of Aberdeen.

Dr. Bastian's remarks on the above subject (*vide* preceding article) will receive the attention due to any deliberate statement made by so high an authority. I question, however, if pathologists will be impressed with either the clearness or the cogency of the arguments adduced by Dr. Bastian against the germ-theory of disease, or will regard as other than rash and unwarranted his statements, "that existing evidence seems abundantly sufficient for the rejection of this doctrine as untrue," and that "all the distinctive positions of those who advocate a belief in the so-called 'germ-theory of disease,' or rely upon the exclusive doctrine of a 'contagium vivum,' seem to be absolutely broken down and refuted." Before we can be in a position to talk so decidedly and confidently regarding "the distinctive positions of" those who maintain the germ-theory of disease, we must have an intelligent appreciation of what those distinctive positions are.

To understand or appreciate arguments for or against the germ-theory of disease, we must have a clear understanding what that theory is. Till we have this, we cannot see the exact force and bearing of a given argument. What, then, is the germ-theory of disease? This is no unnecessary question; for, both by its friends and foes, this theory is referred to in the most loose and indefinite manner. Not only by Dr. Bastian, but by many others, the germ-theory of fermentation, and the germ-theory of disease, are treated and written about as parts of one and the same question. It cannot be too strenuously insisted on that the two questions, though allied, are totally distinct and separate; and that each must stand or fall on its own merits, and independently of the other.

A short statement of what each theory implies will make this apparent. There are two theories as to the mode of production of fermentation. One is the vital theory, according to which living organisms are the sole causes capable of producing fer-

mentative change. The other is the physico-chemical theory, according to which such change may be produced by any organic matter, living or dead, which is undergoing change. All admit the presence of organisms in the fermenting fluid. According to one theory, these organisms are developed from the pre-existing germs whose presence in the fluid gave rise to the fermentative change. According to the other, they are developed *de novo* in the changing fluid.

Such, briefly, are the two opposing views. The question is, not whether living organisms can set agoing fermentation—that is admitted by all—but whether or not living organisms are the sole causes capable of doing so. Of the invariable presence of masses of minute organisms in fermenting fluids there is no doubt. The point of discussion is the source and mode of origin of these organisms. Thus it has come to pass that the discussions which have taken place regarding the mode of production or fermentation (and putrefaction) have really been discussions on the doctrine of spontaneous generation. Those who uphold the germ-theory of fermentation deny the occurrence of heterogenesis, and maintain that all life springs directly from antecedent life. Those who uphold the physico-chemical theory as strenuously maintain the doctrine of heterogenesis.

The germ-theory of fermentation is thus essentially antagonistic to the doctrine of heterogenesis; and the question with which we are really dealing, when we discuss that theory, is the doctrine of spontaneous generation. It is not so with the germ-theory of disease. This theory is that many diseases (notably epidemic and contagious diseases) result from the propagation in the system of minute organisms having no part or share in its normal economy. It deals simply with the competence of living organisms to produce the phenomena of disease; and does not necessarily take cognisance of the question, whether or not organisms may originate *de novo*. The germ-theory of disease, therefore, exists on an independent footing; and would still exist and hold sway, though the physico-chemical theory of fermentation and putrefaction had been proved beyond a doubt. The competence of germs to produce fermentation is admitted by all. The competence of germs to produce the phenomena of disease is the special subject of discussion between the supporters and the opponents of the germ-theory of disease.

I have elsewhere shown that the germ-theory affords of the phenomena of the specific fevers a more complete and satisfactory explanation than any theory hitherto advanced; the whole of the phenomena presented by these diseases being quite explicable on the view that millions of minute organisms are

being propagated in the system during the period of their continuance. And this is the line of argument to which we must sooner or later come, if we would fulfil the highest duties of our calling.

It is not enough to try to explain why infusions of meat decompose, why wine ferments, and why urine becomes putrid. We are constantly surrounded by ailments whose causation and pathology the germ-theory of disease seeks to explain. People are annually dying of these ailments by tens of thousands. It is the investigation of the phenomena of these diseases, as they present themselves at the bedside and in the *post mortem* room, that should engage the attention of physicians and pathologists. Earnest hard-working men of science are eagerly engaged in investigating the more purely scientific, and, to humanity, less practically useful, question of the germ-theory of fermentation and putrefaction. Let us, as medical men, have an equally keen sense of the importance to our science of the allied question of the germ-theory of disease—a theory which deals with the question of the causation and pathology of the most important ailments to which man is liable. But let us free ourselves at once from the fatal error of supposing that our question is inseparably linked with that of the philosophers, or that we are in any way dependent on them.

There are in our ranks earnest, able, and willing workers enough. Let us only have a clear conception of what we are called upon to do. Let us be alive to the great fact that the subjects of our investigations and inquiries are the phenomena of disease, as they present themselves to our notice at the bedside; and let us discard for ever the deluding idea that the germ-theory of disease must stand or fall with the germ-theory of fermentation, or that it has even any necessary connection with it.

What has been said affords all the answer that is required to Dr. Bastian's hostile remarks on the germ-theory of disease. The arguments which he brings forward are all meant to support his well-known views on the doctrines of heterogenesis and archebiosis. They have no bearing on the germ-theory of disease. Dr. Bastian refers to the experiments of Drs. Lewis and Cunningham, and of Dr. Burdon Sanderson. It would not be difficult to show that their facts tell more in favour of than against the germ-theory of disease. On a future occasion, I may take the opportunity to do so. In the meantime, as Dr. Bastian merely adduces them "to show that, under certain conditions, low independent forms of life may originate in the midst of living tissues, previously free from them, by a kind of transformation (heterogenesis) of some of the units of protoplasm, which, though still living, have been modified in nature

and tendency by reason of their existence in a partially devitalised area." As this is all that he adduces them for, I shall content myself with pointing out that he is arguing, not against the germ-theory of disease, but in support of the doctrine of heterogenesis; and that arguments which favour the latter do not necessarily tell against the former.—*British Medical Journal, Jan.* 19, 1878, *p.* 113.

4.—ON THE INFECTIVE PROCESSES OF DISEASE.

By Dr. James Burdon Sanderson, LL.D., F.R.S., Professor of Physiology in University College, London.

[The following is an abstract of the first three lectures of Prof. Sanderson on this interesting subject.]

It is necessary at the outset to have a clear understanding of the meaning of the terms we use, and especially when these terms have been employed for more than one purpose. I have chosen the word "infective" to characterise those diseases which result from the introduction into the system of a poison or poisons. This poison or "seed" may be introduced from without or may originate in some diseased part of the body itself. Probably all of us believe that no contagious disease can originate *de novo*. About some infections there is no doubt—*e.g.*, variola; but about others there is room for some difference of opinion—*e.g.*, suppuration. I cannot but think that Nature, if left to herself, will at once enter upon the process of repair. Believing this, we must regard a wound as a "mushroom-bed," in which infective processes easily take root, and we may hope, by a study of these processes, to prevent or control them. Infective diseases must be looked upon as preventable. I have already pointed out, in a paper read before the Medico-Chirurgical Society, in 1873, that the development of infective matter depends upon the condition of the surroundings of a wound; and that an inflammation that is limited to the tissue affected by an injury is non-infective, while a more widely spread inflammation is infective. In 1873 I said that I thought that microzymes were necessary to infective processes, and as a recent statement of mine has been misunderstood, I must explain that this view does not mean that contagion is brought ready-made into a wound by germs.

The first infective process to which I will direct your attention is septicæmia. I take this because it is best known clinically, and especially because it can be so well studied experimentally. By septicæmia I mean the aggregate of the effects which are produced in the animal organism when putrid matter is mixed with the blood-stream. When a fatal dose of a perfectly clear septic fluid is introduced into the blood of a dog,

the first thing observed is a shuddering, then restlessness, great diminution of muscular power, failure of respiration and pulse—the latter becoming very rapid—and then vomiting, violent tenesmus, with passage of feculent and afterwards muco-sanguineous dejecta. These symptoms are so constant as to be characteristic. If the quantity of fluid employed be just a little below the fatal dose, the most violent gastro-enteritic symptoms pass off, and the animal very rapidly recovers. This proves that the poison has no power to multiply in the organism, and, together with the rapid fall of temperature below the normal in all fatal cases, points to failure of the muscular power of the heart as the cause of death, and not alvine disturbance. The post-mortem appearances are—extravasations of blood under the endocarium, especially in the left ventricle; ecchymoses on the pleura and pericardium; intense injection of the mucous membrane of the stomach and small intestine, with stripping off of the epithelium; and congestion of the spleen and other abdominal organs. These changes indicate a tendency to congestion and capillary hemorrhage, and an intense localised congestion of the gastro-intestinal mucous membrane. This tendency is the expression of a change which the blood itself has undergone; this fluid is darker in colour, its corpuscles are rounded rather than disc-shaped, and collect in clumps, and not in rolls, and many of them are in a state of partial solution in the liquor sanguinis. We can have no difficulty in deciding that these phenomena are the *direct consequence* of the action of the poison of the blood and tissues. Although these symptoms are characteristic of the action of septic matter, other substances, and particularly fresh pus, when introduced into the circulation, may exhibit similar actions.

In passing to the consideration of clinical septicæmia it is necessary, on account of the confusion as to the clinical meaning of the term, to state that what is meant is a disease of the blood, not of any particular organ or texture. I exclude from my definition all characters which relate to the development of metastatic abscesses or secondary foci of inflammation. Not that I question the very close relation between the causes of septicæmia and pyæmia, but it is both possible and desirable to distinguish them, for while the effects of a single dose of septic poison may be transient, the first steps in the development of pyæmia are almost invariably followed by the subsequent ones. Septic matter is very rarely introduced into the body from the outside in sufficient quantity to produce its characteristic symptoms; as a rule the poison which passes into the blood has been elaborated within the organism—*e.g.*, in a wound which has been contaminated from outside. Thus

clinical and experimental septicæmia differ in their mode of origin. The symptoms are difficult to observe; they are insidious in their onset, thus contrasting with the signs of pyæmia. We have initial pyrexia subsiding, in fatal cases, into collapse; while nausea, vomiting, and diarrhœa are seldom wanting, though never attaining such intensity as in the carnivora. Post-mortem we find the same tendency to capillary congestion and stasis of the alimentary mucous membranes, and serous membranes of the heart and lungs, and the same conspicuous absence of inflammatory change as I have already described as met with in the dog. Cases more exactly comparable with experimental septicæmia are seen in obstetric practice, where failure of the circulation, dyspnœa from muscular adynamia, and collapse, ending in death, come on as early as twenty-four hours after delivery. There are no signs of inflammation of the uterus or the surrounding structures, but merely the changes already described. These are instances of blood-poisoning as free from the element of purulent infection as if the patient had a dose of some virulent liquid infused into her veins. Septicæmia, being a constitutional disorder of limited duration, produced by the entrance into the blood-stream of a certain quantity of septic material, is a complication rather than a disease, having no necessary connexion with any local process, and no development; and, the poison producing it having no tendency to multiply in the organism, there is no reason why the morbid process should not come to an end of itself, unless the original dose is fatal, or a second infection takes place.

We will now consider the question, "What is it in septic liquids which renders them virulent, and how can we characterise their virulence?" Here is a perfectly transparent liquid strongly charged with septic poison, prepared by treating putrilage with boiling alcohol, filtering and evaporating, and extracting the residue with water. With reference to this I have to prove to you that it contains neither bacteria nor their germs, but that we have the strongest reason for believing that the virus itself cannot be produced without the agency of bacteria. The microscope does not enable me to say that any fluid is germ-free, for germs—bodies which when sown produce an organism—lie so completely beyond the powers of our present instruments that there existence is for the present matter of inference, not of observation. The only test is a physiological one. I can add a drop of any fluid to be examined to Pasteur's cultivating solution, if it is germ-free the fluid will remain clear, but if not it will become opalescent from the growth of organisms. Tested in this manner, this septic fluid is found to be sterile, and there can be no doubt

that living germs are not concerned in its toxical action. The earlier experiments on septic infection were made with putrilage, and so it was supposed that the septic poison was some product of the putrefactive disintegration of albuminous compounds. But Bergmann has produced the poison without the presence of any albuminous compound. He grew bacteria in a cultivating liquid containing only sugar, ammonia tartrate, and inorganic salts, and found that whilst the first crops were inert, he eventually obtained a particulate liquid possessing all the virulence of albuminous putrilage. In any cultivation of bacteria in a nutrient solution the first crop is always inert, and the poisonous properties are developed later. This may be because time is required for the development of the poison, but, more likely, because the mode of vegetation changes. There is a marked difference in the outward form of the first and subsequent growths of bacteria; the former are mobile rods, the latter are spheroids held together in masses by a transparent substance—the glœa of Cohn. Some years ago Hillier showed that by washing septic bacteria with distilled water, they could be immediately deprived of their toxic properties, although the organisms themselves were as lively as ever. The last fact I shall lay before you is the result of some experiments with antiseptics by Anders. He took a fluid of proved septic properties, and added salicylic acid in quantity sufficient to act as a germicide, and found that it was still as active as before. The same result was obtained when chlorine was used. The amount of antiseptic used was proved by previous experiments to have no physiological action of its own to interfere with the result; and the efficient action of the antiseptic on the bacterial solution was shown by its sterility by the physiological test.

Having discussed the phenomena of septicæmia and the genesis of the septic poison, we are in a position to pass to the consideration of the intimate nature of the action of a septic poison—how the virus acts on the blood so as to produce the characteristic changes which are observed. I shall endeavour to show that as far as concerns all the phenomena which belong to the circulatory system we have good reason for attributing them to the pernicious influence exercised by the poison on the corpuscles, for the marked changes produced in the organs of circulation by poisons which can only act by disintegrating the blood-corpuscles, entirely resemble those of septicæmia. Thus Schmidt has shown that if, in a dog, a couple of ounces of blood are taken from the femoral artery and allowed to coagulate, and after the lapse of twenty minutes or so the clot is broken up and kneaded with the fingers, and the carefully strained blood is reintroduced into the femoral artery of the

opposite side, so as to mix very thoroughly with the blood-stream, it produces disturbances of the circulatory organs exactly corresponding with those seen in septicæmia. To explain these facts it is necessary to refer to what is known of the physiological process of the coagulation of the blood. It has long been known that fibrin does not exist as such in the circulating blood, but that it is formed by the union of fibrinogen and fibrino-plastin, and that the cause of the union of these bodies is the withdrawal of the influence naturally exercised upon it by the living vessels. This influence was till lately an insoluble mystery, but we have now good reason for believing that the white corpuscles contain a ferment which is set free on their disintegration, which causes the union of the two "fibrin factors." The fact of the disintegration of the white corpuscles in the process of coagulation can be directly observed with the microscope. It has also been shown that when a coagulable plasma is filtered at a freezing temperature by which the white corpuscles are separated from it, it loses its power of coagulation. If now the corpuscles on the filter are washed with ice-cold water, and then with a slightly alkaline solution of sodic carbonate, a fluid passes through which, when added to the decorpusculated plasma, again restores to it its original coagulability. Schmidt has shown that all spontaneously coagulable fluids contain, in addition to fibrin factors, this fibrin ferment, which can be obtained from blood-clot by the same process that I adopted to get my septic extract from putrilage. It is found that plasma, containing its leucocytes, is as rich in this ferment as the corresponding quantity of blood, while plasma deprived of them contains no ferment whatever. We can now see the explanation of the poisonous effects of the reinjection of cruor; the disintegrated leucocytes cause intravascular coagulation. In some cases it was found that the animal died rapidly, collapsed and half asphyxiated, from a clot in the right ventricle and pulmonary artery; but, as the poison is intimately mixed with the blood before it reaches the heart, its effects are generally systemic, stopping the circulation close to the capillaries. This experiment also shows us that the selection of the gastro-intestinal mucous membrane by the septic poison is not *specific;* for we may explain it by the difference between the arterial and venous pressures being less in the parts which send their blood to the portal vein than anywhere else except in the lungs. It has been shown clinically, as well as experimentally, that in the intestine capillary embolism occurs with great facility, and the stoppage of a single artery is capable of producing a localised gastro-enteritis with all its accompaniments of capillary congestion, stasis, exudation, and shedding of epithelium. The special affection of the villi is easily

accounted for by remembering that their arteries are necessarily terminal. But we must not infer that the *whole* effect of the septic poison is due to the premature disintegration of the leucocytes; the symptoms relating to the nervous system *may* be due to circulation changes. As regard the pyrexia it seems probable that the septic poison affects all the more active structures of the body, and that in accordance with the view of fever given here in 1872, the pyrexia is the expression of that general damage.

We have seen in a former series of lectures that germs are continually entering the organism, but that they are never met with in healthy circulating blood. They are not eliminated, but probably are destroyed by the action of the living tissues during that period of latency which Tyndall supposes to exist between the germ state and that of the finished organism. These germs also exist in certain organs, as the liver and kidneys, as is shown by their rapid developments after death when means are taken to prevent the entrance of material from outside. In connection with this fact we must also remember that in peritonitis or pleurisy in rodents, produced either by a general infection or the action of an irritant, even a germicide, the inflammatory fluid swarms with bacteria. These facts lead us to the conclusion that, although bacteria are an essential condition in the development of an infective process, they are not the determining condition; this must be sought in the irritant or the septic poison.—*Lancet, Dec.* 22 *and* 29, pp. 936, 968.

5.—THE GERM THEORY, CONTAGIUM VIVUM, SPECIFIC INFECTIONS.

By Prof. J. Burdon Sanderson, M.D., LL.D., F.R.S., &c.

[We extract the following from Dr. Sanderson's fourth lecture "On the Infective Processes of Disease."]

By the expression *contagium vivum*, I mean to designate the doctrine that, when a contagious disease is communicated by the atmosphere, by personal intercourse, or in any other way, that conveyance takes place by specifically endowed organisms, which stand in a similar relation to the disease to that in which the seed does to the plant. For, just as the seed is at once the origin and offspring of the plant, so, according to this view, the morbific germ produces the disease, and is in its turn produced by it. This being understood, it is evident that the term contagium vivum relates exclusively to specific diseases, and therefore belongs mostly to medicine.

For a similar reason, the *germ-theory*, which relates to the inflammatory and other consequences of injuries, involves questions which are almost exclusively surgical. That theory

teaches that certain organised and living particles of extreme, if not ultra-microscopical minuteness, which are always suspended in the atmosphere, are in such senses, the causes of the suppuration and other destructive changes which interfere with the healing of a wound, that (1) if these organisms are excluded, the wound must enter at once on the processes of reparation, and (2) that if these organisms are present, it cannot so heal; and, consequently, that the whole secret of the successful treatment of wounds consists in the exclusion of that finest form of atmospheric dust which, whatever be the size of its particles, has been identified as well by physicists as by mycologists, with the contaminating agents in question.

When, as at Halle, we have pyæmic affections banished from wards which were formerly infested; when, as at Munich, hospital gangrene (a disease which probably a great many of us have never seen), which up to 1854 affected 80 per cent. of the traumatic cases in the hospital, has ceased to exist; when we see the same experience repeated at Strasburg, at Leipsic, at Innsbruck, and I know not at how many other places, we need no longer wonder that it is common to hear the discovery of Lister spoken of in Germany as the greatest progress in the art of medicine which has taken place in modern times. But it is not, as I said before, with the human life saved and human suffering spared by his improvements that we have to do at present, but with the pathological lesson to be learnt from these results.

The germ-theory explains everything by saying that the reason why a wound goes wrong is, that atmospheric particles enter in and contaminate it. It compares, in short, the treatment of an operation-wound with one of Professor Tyndall's or M. Pasteur's experiments on spontaneous generation. It professes, indeed, to be an application of the principles and method of such an experiment to a practical purpose. It is perfectly true that the same fastidious precautions which are necessary to command success in the one case are also necessary in the other. What, then, is the difference between them? In the laboratory experiment, the criterion of success is the absence of living organisms. If they appear in the test-liquid employed, the operation is considered as a failure; for, so far as experience has yet taught us, there is no case in which organisms come into existence otherwise than as the progeny or offspring of previously existing bacteria. Are we justified in applying the same test to the antiseptic treatment of an injured surface? From the theoretical point of view, we certainly are; for if, on a wounded surface protected by antiseptic dressings, bacteria were to appear, then one of two things would be certain: either that the germs had entered from outside, or that they existed in the patient's organism previously.

But is this the true criterion? Certainly not. In judging of the value of a therapeutical method, the one and only criterion is success. The question of primary importance is not whether bacterial germs are killed or sterilised, but whether the pathological results of septic infection are prevented, so that the wound is free from inflammation, the constitution free from fever. Consequently, whatever doubts we may be led to entertain as to the theory, those doubts ought not to interfere with our cordial acceptance of the evidence which has now been accumulated of its prophylactic value.

Some of you are probably aware that two series of observations have lately been published in Germany which go to show that, although in many instances the occlusive treatment is successful, not merely in warding off infective results, but also in the shutting out of germs, there are others in which the first object is completely accomplished—the patient remaining free from fever, pain, and inflammation—but yet organisms exist in the discharges. Obviously we might, if we chose, say, "Professor Lücke is not Professor Lister. It does not follow that, because organisms are found in the discharges from drainage-tubes at Strasburg, they should also be found at King's College." This, of course, may be freely admitted, but does not answer the objection. What is alleged is, that in these cases, although the method was carried out sufficiently perfectly to accomplish its end (the absence of pain, fever, inflammation), nevertheless organisms had found their way into the discharges; but that here, as elsewhere, being placed under conditions hostile to their development, they had been harmless.

If this be true, the inference to be drawn from it is, of course, that the treatment of a wound is not an experiment on spontaneous generation, but an experiment on infection; to use Professor Tyndall's striking expression, a combat not with atmospheric germs, but with pathological infectivity. Clearly, if it were purely a question of the exclusion of germs, the entrance of one germ would be fatal. But are there not hundreds of instances in which traumatic surfaces are exposed to germs without any effect whatever? Look at the ordinary results of the thousands of operations that are daily performed on animals for economical purposes without any precautions against germs. How often does infection occur? Not once in ten thousand times. Again, how often does it happen that an ordinary incised wound, notwithstanding that the knife that made it is beset with germs of many sorts, fails to heal straight off *per primam?* Again, in vaccination and in subcutaneous injection, do we not take every pains to do the operation in such a way as to promote absorption of whatever is on the surface of the instrument; yet whoever thinks of air-germs in

connection with either of these operations? Dangers from infection, alas! attend both; but the infections we have to fear are of *pathological* not *meteorological*, origin.

Now, these facts cannot be explained on the ground that the wounds in question are too inconsiderable; for we know by experience that, when real infection is in question, it is precisely by such wounds that it enters. For example, you will find, if you examine the records of outbreaks of hospital gangrene, that the wounds attacked were not suppurating or granulating wounds, but mere scratches or pricks.

I am desirous, in all that I say on this subject, to keep strictly within the limits of my own scope; I do not desire to make practical applications. My aim is to show that the principles which underlie the success of the antiseptic treatment are deeper than the surface of the wound. Let me, in conclusion, endeavour to indicate what those principles must be. The first principle that suggests itself on pathological grounds relates to the avoidance of conditions in the wound itself which favour the development of infectivity. Considering that the development of infective action in a wound (supposing it to depend on the evolution of successive generations of organisms) must be a question of time, and that the circulating blood and living tissue are the most powerful colytics that we know in restraining and preventing that development, all methods which tend to prevent the accumulation of blood or liquids in wounds must be of great importance, not because these materials are in themselves incapable of being absorbed or organised, but because, if accumulated in quantities, they are withdrawn from the colytic influence of the living tissues.

The second principle I take to be the avoidance of infective contamination, in connection with which the question at once arises of the source of such contamination. If it be not atmospheric particles, what is it? Bear in mind that, wherever bacteria have been vegetating for some time in moisture containing the material for their growth (and we have seen how simple their requirements are in this respect), there infective virus is being elaborated. Consequently, that ordinary filth (of which the only scientific definition is, that it consists of the products of bacterial evolution) is more or less virulent or infective according to its development. Hence, if against all filth there lie a *primâ facie* presumption that it is infective and a source of danger, scrupulous cleanliness must be essential. I need not add that, in antiseptic surgery and in the practice of those surgeons who have accomplished the greatest successes in the combat with traumatic infection, this principle is so well recognised that, in the future, we may hope to speak of "surgically pure" instruments as familiarly, and with as much

significance, as we now speak of "chemically pure" substances. As may have been observed before, the colytic treatment is an importation of the methods and principles of the laboratory into the surgical ward.

But there is a third principle, which must not be forgotten. If there be any truth in what I have been saying as to the mode of origin of infective virus, the use of disinfectants, and among disinfectants of those which are known by experiment to be the most efficient colytics, must be of the greatest value. The benefits already derived from the use of carbolic acid are matter of accumulated clinical experience, and, as I have already hinted, even better results may be hoped for from other bodies belonging to the same category.

I have occupied so much time with the discussion of the origin of common, or traumatic, or phlogistic infectivity, that I have too little left for the other question I have to bring before you: namely, that of the intervention of living organisms—microphytes—in specific infection, *i.e.*, in the communication of specific diseases from diseased to healthy individuals, and in the development of the pathological consequences of such communication. This question, let me observe, although quite as important as the other, is so entirely unconnected with it, that no conclusions we have arrived at with respect to the origin of traumatic infection can be applied without reconsideration to that of *contagium vivum*. The septic poison we have seen to be an exclusive product of bacterial development, a product which bacteria are capable of manufacturing from unorganised and perfectly harmless material, a product which, although incapable of passing through certain kinds of filters, is soluble in the ordinary sense. Pathologically, we have seen that it does not act the part of a specific contagium; that, in order to the production of its morbific effects, a sufficient quantity must be introduced into the circulation; and, further, that the intensity of effect is proportionate to the quantity introduced; so that if the amount be not too great, the tendency is to a favourable, not to a fatal, termination. But every one knows that there occur from time to time in clinical experience instances of a sort of septicæmia of a much more virulent kind; cases, for example, of septic peritonitis, in which the quantity of the agent required to produce the fatal result is not measured by drops or grains, but (if one may so express oneself) in homœopathic doses; cases in which we at once recognise that we have to do, not with a poison of which the effect is determined by its quantity, but with a ferment of which the destructiveness to life is chiefly dependent on the rapidity of its development.

The history of these most malignant forms of septicæmia (I

call it so for want of a better word) is simple enough. The gateway by which the seed enters may be, and usually is, a mere prick, and the primary effect so light as to excite no attention. After hours, or even days, the absorbents and lymphatic glands, of which they are the tributaries, become the seat of inflammation, on which follows diffuse suppurative infiltration of the surrounding cellular tissue. Soon the process extends beyond the limb affected, to the integuments of the trunk, to the pleura, to the peritoneum; the inevitably fatal result, which is preceded by delirium and collapse, being partly, perhaps, due to the direct influence of the specific contagium, but principally to the enormous development in the organism of the ordinary septic poison.

These appalling cases must be clearly distinguished as regards their pathological nature from ordinary septicæmia. That they also have their mycological interpretation I have endeavoured to show by experiment. I have shown, as regards peritonitis, that if the exudation of a simple peritonitis be injected fresh into the peritoneum of another animal, the disease assumes a more intense form in the second than in the first; that if in this way the disease be communicated to several animals, the effects will differ in different cases; that if by artificial selection the most severe case be picked out and the exudation from that case be used for further transmission, a still more intense inflammation will be the result, until at last a virus is obtained of which the virulence resembles that of the specific cases of malignant peritonitis in the human subject to which I have been referring. To account for the production of such cases, you have only to substitute accidental for intentional selection, the most important point to bear in mind being that the process by which the poison is developed, whether accompanied by a specific organic form or not, is a pathological process, that is, a process which can only go on in the living tissue, and which is necessarily associated with a certain definite succession of structural and physiological changes in the affected part. To obtain information respecting these processes, we must have recourse to experimental pathology. For example, experiments like those to which I have referred, which show how, by a gradual evolution, we may rise from common traumatic infectivity to the intensified virulence of malignant septicæmia, teach us what we could not learn otherwise.—*British Medical Journal, Feb.* 9, 1828, *p.* 179.

6.—REMARKS ON DR. SANDERSON'S LECTURES ON THE INFECTIVE PROCESSES OF DISEASE.

By the EDITOR OF THE BRITISH MEDICAL JOURNAL.

[In the two preceding articles we have given abstracts of these admirable lectures, to which we here add the following interesting comments.]

From a careful study of the lectures, we believe that three facts of especial importance may be noted. 1. Ordinary bacteria are in themselves innocuous when introduced into a healthy organism, probably because of a colytic action of the blood and tissues. 2. When bacteria are provided with fluids, either in or out of the system, which have lost their colytic action, they, as the yeast-plant in the presence of grape-sugar sets up the vinous fermentation, by their growth and development set up fermentative changes, the result of which is the production of a virus, which, when it finds its way into the blood-stream, causes septicæmia. 3. The growth and development of specific organisms in the system cause specific diseases. Keeping these facts in mind, it is evident that it is of the greatest moment that septicæmia, in its cause and effects, be understood to be something very different from such a specific disease as splenic fever; and that the theory employed to explain the *rationale* of the latter—the theory of *contagium vivum*—is very different from the germ-theory.

Septicæmia, being the result of a virus elaborated by bacteria, a virus which does not increase in the system, is only fatal when a sufficient quantity has been introduced; whereas such a specific disease as splenic fever results from a specific organism which increases in the system, and proves fatal when it has sufficiently multiplied. Every one is now so well acquainted with splenic fever and with the already famous organism Bacillus anthracis, which produces it, that it would be out of place at any length to refer to it; but, as it is the first specific disease which we can without doubt or hesitation attribute to a specific bacterium, and must ever be alike interesting to the physician and the scientific investigator. Since proving that, when an animal is inoculated with the spores or rods of Bacillus anthracis, splenic fever is sure to result, Mr. Lister has shown that another specific organism—Bacterium lactis—always leads to a particular fermentation, the lactic acid fermentation; and, just recently, Dr. Klein has made out that the so-called pig-typhoid is produced by another specific organism similar in its growth and development to the bacillus above mentioned. If we have at last found a bacterium which we can see and handle, which we can watch passing through all its phases on the warm stage of a microscope, and if, at any moment, by introducing

it into an animal, we can set up a distinct disease, and one only, may we not hope that, at no distant period, Dr. Sanderson may be able to tell us, in a similar course of lectures, that many of the maladies met with in every-day practice are due to similar causes; that many of them are the result of the growth and development of specific organsims?

Dr. Sanderson, having considered the effects of septicæmia, passed on to the causes; and, we think, clearly made out that probably all but the nervous symptoms and pathological appearances accompanying and resulting from the introduction of a virus into an organism, are due to the disintegration of the colourless blood-corpusles, leading to the formation and deposit of clots in the smaller vessels of the mucous membranes.

Another important consideration was the relation between septicæmia in the lower animals to the septicæmia we have accompanying—*e.g.*, a compound fracture in man. The chief difference is, that the virus, instead of being produced outside the system, it may be from an infusion of muscle in a glass beaker, is elaborated at the seat of the fracture and reaches the blood through the lymphatics. Here, as before, two things are necessary: in the first place, we must have tissues or fluids which have lost their colytic action; and into contact with these, we must bring bacteria. Dead tissues alone are not enough; for, if kept free from bacteria, they remain unchanged. Bacteria alone are not enough; for, without dead tissues, they remain inactive.

This naturally leads us to the germ-theory, which teaches that putrefaction is the result of fermentative changes set up in a wound by living particles always found in the atmosphere. We may discern in the course of time that there are different kinds of putrefaction, and that each is set up by a special organism, just as the lactic acid and vinous fermentations are caused by specific ferments.

For the antiseptic method of treating wounds, no one has said so much in this country as Dr. Sanderson; but we fear much of the good which might have resulted from his strong testimony in its favour has been crippled by the statement which followed, showing that he was not able to endorse the theory on which the method has been built up. Dr. Sanderson, if we understand him aright, thinks that bacteria have not so much to do with putrefaction in a wound as the organised fluids and tissues in and around the wound itself; yet he believes that it would be impossible to have putrefaction without them, just as it is impossible to have septicæmia without them. In a fracture, compound or simple, there are always broken down tissues enough and extravasated blood enough, so that it is only necessary to allow bacteria to come into contact with these in

order to have fermentative changes set up; but it seems that Dr. Sanderson believes that bacteria are always present; hence we should always expect fermentative changes to take place even in simple fractures, and that, if for a short time a wound be exposed to the ordinary air, no harm would follow; and, further, that heaping layer after layer of gauze over a wound cannot be of much use. Why endeavour to keep bacteria out when they are already present? But, if present, how is it that Mr. Lister cannot find them? How is it that clots of blood lying in the wound becomes organised instead of breaking down, as they always do when, even for a few minutes, they are exposed to the ordinary atmosphere?

Dr. Sanderson offers no explanation, but states that the principles which underlie success are deeper than the surface, and ventures to suggest that free drainage, in order to prevent infectivity; scrupulous cleanliness, so as to prevent the organisms from finding a convenient substratum to develope in; and, lastly, the use of disinfectants, may account for the wonderful results obtained.

Those who think with Dr. Sanderson bring forward in favour of their views the statements made by Volkmann, Fischer, Ranke, and others, which go to prove that bacteria are found in the discharges from antiseptic wounds, and say, further, that the existence of bacteria in the kidneys and other abdominal organs is inconsistent with the germ-theory.

In answer to the latter statement, seeing that healthy blood, when kept from the air, like milk, does not putrefy, and that healthy tissues have a colytic action, may they together not so act on the bacteria that enter them from the liver and kidneys, that they prevent their growth and development? May they not be looked upon as so many layers of antiseptic gauze protecting the wound from *within*, as the gauze over the wound protects it from the bacteria *without*? If it be not so, how can we explain why one abscess should remain aseptic and free from bacteria when another abscess, only a few inches removed from it, is teeming with bacteria? And how can we explain why for months a psoas abscess should remain aseptic; for there is no drainage before it is opened, and no colytic of any kind comes into contact with the cavity of the abscess after it is opened?

After *bistournage*, there was no putrefaction unless bacteria—not a virus simply—were introduced into the blood-stream, and why then? May it not be that so many bacteria were introduced at a time that they were deposited amongst the degenerated tissues before the blood was able to render them inactive? If the bacteria were introduced some time previously to the twisting of the cord, they might be rendered inactive

before a convenient substratum was provided for them, and thus no inflammation, pain, &c., would ensue.

If, then, bacteria do exist in antiseptic wounds, and if they cannot, in healthy patients, reach the wound in an active condition from within, they must from without; but, seeing that, in absolutely aseptic cases, no bacteria are found, we must conclude that, when they were present, it is for the same reason that the most careful experimenter sometimes finds them in flasks which, had sufficient care been taken, would have remained sterile. Some time ago, one of the writers who stated that bacteria were found under antiseptic dressings, confessed that he had mistaken small innocuous particles for active living organisms. That bacteria are sometimes present even in Professor Lister's cases, we do not for a moment doubt; but their existence can generally be accouuted for, and their presence is invariably accompanied with slight indications of fermentative changes; and such cases, although the results are nearly as good as need be desired, are never to be considered absolutely aseptic. From what we have seen of antiseptic cases, so called in London and on the Continent, we are not surprised to hear that bacteria are sometimes, yea often, if not always found; that results at all approaching the best results of Mr. Lister are obtained even in the presence of bacteria, is a still stronger argument in favour of antiseptics even in a modified form.

Every one knows that good results are obtained without following Lister's method; but, as Professor Tyndall has said, the good results are obtained because the surgeons in principle carry out or have anticipated parts of that method; but those acquainted with London hospitals know well that, notwithstanding all the care and cleanliness, pyæmia is far from being extinct.

In answer to the statements about drainage, cleanliness, and the use of carbolic acid, we must say that, in several hospitals, we have seen the freest possible drainage practised, the wounds kept scrupulously clean, and quantities of carbolic acid used, not to mention piles of gauze, and yet no better results than might have been obtained by leaving the wound exposed to the air or simply covered by a layer of dry lint. Nurses and dressers well know that the scrupulous cleanliness for which Mr. Lister so often gets credit, almost entirely exists in the imagination of those not *practically* acquainted with the method. In wards where the antiseptic treatment is not carried out, or where it is practised in a mechanical way, double the care and oft-repeated dressings at the best produce results which cannot for a moment be compared with those far more easily obtained by practising in an enlightened way the antiseptic method.—*Brit. Med. Journal, Feb.* 23, p. 264.

7.—THE BASES OF THE ANTISEPTIC SYSTEM.

By the Editor of the Medical Times and Gazette.

The attention of the profession has been forcibly called of late to the doctrine and practice of Antiseptic Surgery. The address of Dr. Roberts, before the British Medical Association, in August, presented the subject from the point of view of the practitioner of Medicine, whilst the oration of Dr. Allen Thomson before the British Medical Association, delivered shortly afterwards, dealt with the doctrine as it commends itself to the man of science. Still more recently, Professor Lister has added to the experimental data which support the theory; and the presence of this apostle of the antiseptic doctrine in the midst of the surgeons of the metropolis will, no doubt, give zest to the interest which the subject so generally inspires. It appears to us that the present is no inopportune moment for a retrospective glance at the bases, theoretical and practical, on which the system is founded, for it is more than probable that errors exist, not only as to its scientific correlations, but also as to the steps by which it was established and perfected.

There is no doubt that the great theoretical basis of the Antiseptic System was laid by Pasteur in 1860. Disregarding all anterior observations, which savoured more or less of hypothesis, we may look upon the experimental investigations of the French chemist as establishing, with the nearest approach to scientific precision, the germ-theory of fermentation. By such is meant the doctrine that fermentive decompositions are brought about, not by any occult property of changeable organic matter, but by the direct agency of living organisms. It is needless to say that this doctrine has been very strongly contested and severely criticised ever since. M. Pouchet at first, with a very strong following, opposed to it the theory of Heterogeny, the teaching of which was that the organisms which attended fermentations were spontaneously developed in putrescible fluids, and were not disposing agents. Subsequent observers have announced their adherence to this latter doctrine with various modifications, but it is scarcely too much to say that their followers are a vanishing number; for rigid scientific investigation has tended more and more to establish the points—first, that fermentive and putrefactive decompositions are closely correlated with the presence of minute living organisms; and secondly, that the absence of such living particles from the most highly decomposable fluids is strictly in relation with the efficiency of the means employed to exclude them. It is needless, however, to dwell on these points, for the germ-theory of fermentation and putrefaction is a necessary postulate to the antiseptic system. If that be not conceded, the system can have no scientific existence.

We read in Professor Allen Thomson's address that Professor Lister "had the merit of being the first to apply the germ-theory of putrefaction to explain the formation of putrid matters in the living body; and he has founded on this theory the now well-known antiseptic treatment of wounds, the importance of which it would be difficult to over-estimate." With every disposition, however, to accord to Professor Lister the distinction of having elaborated and perfected the method, we cannot conclude from a review of the history of the subject that he initiated it.

Let us turn to what we may term the practical basis of the antiseptic system in surgery. We may define this as the method whereby putrefactive decomposition on the surface of wounds of the tissues is prevented, and thus a great following of evils and dangers abrogated. The disinfection of wounds is no novel proceeding. In 1859, M. Démeaux employed a powder consisting of a mixture of tar and plaster-of-Paris (a compound which had been patented the year previous) for disinfecting wounded surfaces. In the same year MM. Lemaire and Lebœuf used an emulsion of tar, prepared by means of saponine, for the like purposes. This saponine was extracted from the bark of *Quillaya saponaria*, and was found to have the property of suspending in a very perfect manner the insoluble tar in the form of a saponaceous emulsion. Velpeau reported against it, but, nevertheless, its use gained ground, and in 1862 the Administration of Civil Hospitals in Paris authorised its employment in all the establishments. It may be urged, however, that it was employed just as hosts of other lotions and applications had been used previously—to keep wounds "sweet," and with no special reference to the doctrines of putrefaction, or the question of the advent of septic germs; but this could not be said in 1860, for in the same year that Pasteur communicated his thesis to the Academy of Sciences, Lemaire published a pamphlet ("Du Coaltar Saponiné"), in which he narrated eighty observations made on the human subject and on animals, and in which he distinctly defined the objects of the application of the coal-tar emulsion to be (1) the prevention of putrefaction by direct action upon the septic germs, and (2) the arrest of the production of pus. Lemaire showed that the active agent in the tar was carbolic acid, and in 1863 he published his work entitled, "De l'Acide Phénique, de son Action sur les Végétaux, les Animaux, les Ferments, les Vénins, les Virus, les Miasmes; et de ses Applications à l'Industrie, à l'Hygiène, aux Sciences Anatomiques, et à la Thérapeutique." A second edition was published in 1865. In this work it is not too much to say that the antiseptic method in surgery was distinctly inculcated. The antiseptic treatment

of wounds, comminuted fractures, burns, and necroses, is fully described. After having cited a case of compound comminuted fracture of the bones of four-fingers, successfully treated, Lemaire adds—"This observation appears to me to offer an important teaching as regards surgery. In fractures with crushing, amputation is recommended. Here we have seen that, in spite of cold-water dressing, counselled in like cases, for twenty-four hours the suffering kept increasing. It is almost certain that, if I had contented myself with this treatment, suppuration would have been established, and amputation have become inevitable. Coal-tar emulsion being employed, the pain is at once relieved, and after twenty-four hours rendered null, cicatrisation of bones and soft parts is effected, and everything preserved. This good result obtained in the case of four bones not only evidences the remarkable properties of coal-tar, but appears to me to indicate the employment of this method in cases of fractures of the limbs with crushing, before having recourse to operation." Lemaire is also explicit as to the efficacy of antiseptics in controlling suppuration. He asserts that he can arrest and reproduce at will the formation of pus, just as he can arrest and reproduce fermentation and germination; and he adds, "If my theory be true, we ought to be able to prevent the formation of pus by the use of emulsion of tar before the tissues are involved in inflammation."

Here, then, we have both *ratio* and *modus* of the antiseptic system. We have yet to consider, however, the precise relation supposed to subsist between suppuration on the one hand, and putrefaction, fermentation, or the access of germs on the other. On this point Lemaire expressed very decided views, though most of us will probably think that he passed far beyond the just limits of scientific inference. He considered that the formation of pus was directly due to the advent of germ-laden air, that the globules of pus were comparable with those of yeast, and that they had an analogous function and an identical origin.

Whatever view we may adopt as to the origin of pus, we must agree that it is no material which is, by origin and development, foreign to the organism: it is the degraded protoplasm of the organism itself. The pus-cell, however, though possessed of active vitality, does not minister to the nutritive needs of the organism in which it is developed, but, by multiplying and augmenting, acts as a foreign body, and, pressing on the surrounding tissues like a mechanical agent, causes progressive necrosis. Pus-cells indicate, according to Professor Lister, "the extreme of excess of quantity and impairment of quality in the product of abnormally excited nutrition." We have abundant evidence that the excitation due to putrefactive

decomposition is a most potent and fatal cause of pus-formation, but we cannot hold that it is the *sole* exciting cause. We know that pus will form as a consequence of direct violence, and in situations where the advent of germ-laden air is in the highest degree improbable. We know, moreover, that the very agents which in dilution prevent pus-formation, when too strong can induce it. It seems, therefore, only reasonable to conclude that putrefaction, though probably the chief, is not the only exciting cause of suppuration.

History, then, teaches us that the authors of the Antiseptic System were Pasteur and Lemaire, who formulated its principles in 1860. It was not till 1867 that Professor Lister made public his method fonnded on these principles, but it will be agreed on all hands that he has done a most valuable work in elaborating with all the earnestness of a truly scientific observer both the theory and practice of antiseptic surgery."—*Medical Times and Gazette, Jan.* 5, 1878, *p.* 10.

8.— DR. TAQUET ON HEREDITY IN ALCOHOLISM.

Reported by Dr. Charles Aldridge.

Montesquieu has said that the strength of a nation depends upon the number of men it can call into the field when threatened by an enemy. The history of the late disastrous war showed that in spite of courage and arms of precision this remains true, as the victory was with the large battalions. Whence this physical decadence? The year 1873, compared with 1872, shows an increase of 51.523 on the total mortality, and a diminution of 19.636 on the birth rate. M. Taquet would place the abuse of spirits in the first line of causes which tend to depopulation. If the drunkard were the only sufferer by his excess, consolation would be easy; but as Lancereaux has said, alcholism is not only a disease of the individual, but is a family disease, and projects its evil influence upon the race. M. Rousel says, "Absorbed by a taste which quickly transforms itself into an irresistible need, one sees alcohol impregnate strongly the organism, alter the radical forces, and degrade little by little the physical and moral nature of the man. One sees it follow the individual in his offspring, his family receiving from him a fatal heritage in debility, deafness, a crowd of nervous disorders, moral imbecility, idiocy, mental alienation, and wicked instincts. The Indians of America have disappeared before the destructive powers of alcohol, when fire and sword failed to vanquish them. Nor is this fact new in history, for the legislation of Lycurgus favoured drunkenness in the conquered, in order that their healthy aspirations might be dulled, and that they might the more easily submit to slavery."

In alcoholism, as in all affections which are transmitted from ancestors to descendants, we may recognise the heredity of similitude, as well as the heredity of change. The heridity of similitude presents itself in two aspects. It remains latent, and requires the influence of example and circumstance to awaken it, or it may burst forth in a sudden manner without seeming cause. Esquirol reports a case where the death of a grandfather and father had quickly followed their thirst for drink, in which the little son at five years of age showed a decided taste for the same kind of drink. M. Taquet knows of a case where a person died early from alcoholic excess, leaving an infant, who at a very early age showed a decided tendency to intemperance, until now, at maturity, he has developed a partiality for the same drink which his father loved. Fusch speaks of a dissolute drunkard who came to his end after having plunged his family into profound misery; two of his sons early presented the same vice, and the third, after remaining sober until his thirtieth year, suddenly drank in a violent way. Of suicide associated with alcohol, the history of the family Dufray presents an interesting example. It consisted of four brothers who were addicted to the most excessive drunkenness and licentiousness. The eldest drowned himself, the second hanged himself, the third cut his throat, and the fourth threw himself from a third storey and was killed. Drunkenness is a complex state, being generally accompanied by physical or nervous disturbances, as will be seen in the following example.

Observation I.—The head of the family was a drunkard and a debauchee. His wife was remarkably sober, although the daughter of a drunken father, and sister to two youths who both had inherited their father's vice. Of this marriage were born three boys and two girls. The eldest is as immoral as his father, and presents an organic lesion of the heart. He married a wife who seems to offer nothing abnormal. They had three children, two girls and a boy. The eldest manifested violent sexualism at an early age, and gave birth to a hydrocephalic child to an unknown father. The second girl is almost as dissolute as her sister; and the boy is quite imbecile, epileptic, and a drunkard.

2. The second son has been treated twice in an asylum, for mania with homicidal impulses.

3. The third son, after an existence of debauchery and pleasure, died at the age of 21 years, of consumption, hitherto unknown in the family.

4. The eldest of the girls has been married for 12 years to a sober, intelligent man. Out of six of their children the heredity has fallen upon one, who is drunken, licentious, and a thief.

5. The youngest daughter has lost all moral sense and decency, leading a most irregular life, although well married.

This observation presents two interesting considerations, viz. :—

1st. Sexual desires show themselves early in the children of drunkards, and are associated with an absence of moral sense.

2nd. Phthisis, when not heriditary, is capable of being produced by spirituous excess. Magnus Huss and Launy have supported this thesis by numerous examples.

Observation II.—The father died of cerebral softening, determined by alcoholic excess. The mother died of ascites; cause unknown. The result of this marriage was one daughter, who married a man who has no trace of hereditary disease. They have had six children: 1st. An idiot, born blind; 2nd. An imbecile; 3rd. An imbecile; 4th. Imbecile, and born blind; 5th. Well gifted, morally and physically; 6th. Born an idiot. Here we find the evil influence passing over the immediate descendants and attacking in various ways the next generation.

Observation III.—The grandfather was a drunkard, which is all that is known of him. His wife died of cancer; an only son, a rough and violent fellow, died of alcoholism in an asylum for the insane. The son of this latter was of an extremely impressionable nature, not able to bear the sight of any cutting instrument, and was liable to be thrown into a state of nervous excitement at the sight of a soldier or an armourer's shop. He married, and since has had an attack of mania, during which he attempted suicide. He has had three children, of whom the eldest died soon after birth; the second, not yet two years old, presents nothing worth notice; the third was hydrocephalic, and died in convulsions.

Other things being equal, the hereditary transmission, to whatever order it appertains, will be more surely by the mother than by the father. The heredity influence of the mother is noted by Esquirol in the physiognomy, in the conformation of the body, the habits and predilections. Baillarger and Dagonet support this, and Gintrac says, "The mother exercises a double action—one which she shares with the father in the conception, the other which is proper to her, and which depends upon the relations established during the intra-uterine life between her and the product of conception. For this reason intemperance in the female, if it be not passing, will have in all cases exceptional gravity. The children of female drunkards, if they escape the morbid influences which compromise their existence in the womb of their mother or at birth, are often idiots, imbeciles, insane, or epileptic. These divers affections are the consequences of cerebral congestions, of hemorrhages into the membranes or nervous substance, of encephalitis, of softening

chronically, determined by the abuse of alcohol by the parent." Of all the manifestations of alcoholic heredity, epilepsy is believed to be the most important and the most common. Of 95 epileptics examined, M. A. Voisin found twelve who had scrofulous and true tubercular antecedents; 12 had ancestors who died from alcoholic excess, or were subject during their honeymoon to excessive abuse of alcohol. Marcet reports of a drunkard who had 16 children, that five were dead and the remainder epileptic. One, G., who was proved to have been in a state of constant drunkenness for some time, had a child born to him, who from its youngest infancy had convulsions, and is now a confirmed epileptic. We believe that convulsions in infancy are neither more nor less than incomplete signs of epilepsy, and that they predispose singularly to mental alienation. Van Swieten has said with reason that perhaps there is not a lunatic who has not had convulsions in his infancy.

One other accidental consequence of drunkenness is that it diminishes the elements of vitality in the child, so that it comes into the world with but half an existence, so that the least blast falling upon it will prevail.

That alcohol carries into the composition of the fecundating material modifications of which we are ignorant, must be admitted. In fine, we would point out hydrocephalus in the offspring following alcoholism or simple drunkenness in the parent. The children of drunkards are not all of necessity idiots, lunatics, or epileptics, but there are few that present nothing abnormal; and in those of seeming freedom the germ may be late in developing itself. It is not rare to find precocious cerebral excitement displaying itself most frequently in a good memory. They are parrot-like, and display a remarkable aptitude for some particular pursuit. It, will, however, often be found that they do not fulfil the promise of their early years, seeming to have produced in their youth all of which their organisation is capable. Some find their way early to the gaol, others are eccentric in all their ways and beliefs, and constitute the class of pretentious imbeciles. Nature would seem to have a horror of the anomalies and monstrosities that alcoholism induces, so that it often rejects from the womb. Darwin tells us that the families of drunkards become extinct in the fourth generation, after having descended through the scale of physical and intellectual degeneration.

Dr. Taquet concludes by remarking that the children of drunken parents are more liable to attacks from prevailing epidemics and sink under them sooner than other people.—*London Medical Record, Jan.* 15, 1878, *p.* 8.

9.—OBSERVATIONS ON ANTIPYRETICS.

By Dr. John A. Erskine Stuart, Prestonpans.

During the past year an immense number of cases of rheumatic fever, typhoid fever, and other febrile conditions, have been treated successfully, and carefully noted in the medical journals, the antipyretic system of treatment by salicylic acid and its congeners being employed. Almost no explanation of the action of these drugs has been attempted by writers in this country, but from abroad we have many papers on the subject. At a meeting of the Edinburgh Medico-Chirurgical Society held on Dec. 6, 1876, an interesting discussion on antipyretics arose on a paper on the treatment of an epidemic of typhoid fever by antipyretics, by Dr. George Hunter, of Linlithgow. A number of medical men were present who had used salicylic acid and its congeners largely in various cases, and a great difference of opinion seemed to prevail regarding the *rationale* of their action. My object, therefore, in writing this paper, is to try to explain by a few observations of my own, which may be taken for what they are worth, how these drugs act. I shall, therefore, first take up the vegetable antiperiodics, and then salicylic acid and its congeners, and strive to show that they act in precisely the same way, and that they are not only internal antiseptics, but that they have a certain action on the vascular system.

Vegetable Antiperiodics.—These are quinine, salicine, and bebeerine. Binz is of opinion that quinine is an antiseptic, and a protoplasmic poison; also that it prevents the emigration of leucocytes, and restrains the dilatation of bloodvessels. Jerusalimsky's views are somewhat similar. He found that the red corpuscles became larger under the influence of quinine; that small doses act as tonics to the heart, while large doses cause paralysis of that organ, and cause the blood-pressure to sink rapidly. The question with which we have to deal at present is this—How does quinine reduce temperature? Is it by acting on the germs of disease, or by some peculiar action on the heart and bloodvessels? I have proved that quinine, salicine, bebeerine, in solution are incapable of restraining the putrefaction of fresh urine exposed to the atmosphere, but that when administered internally they are capable of reducing the temperature in febrile conditions to a most marvellous extent. It is, therefore, more than probable that quinine is transformed into some antiseptic substance in the blood, and that this body exercises a paralysing effect on the microzymes in the blood. It is now well known that a large dose of quinine is as powerful in reducing high temperature produced by exercise, as it is in cases of fever presumably due to the presence of germs in the body. It is, therefore, a matter of certainty that it pro-

duces its antipyretic effects not only by exercising its antiseptic action on the tissues, but also by its action on the bloodvessels in raising the blood-pressure,—a low blood-pressure, according to Ackermann, leading to high temperature and increased tissue-change. An excellent application of this latter theory is carried out in the treatment of sunstroke, or heat apoplexy, where paralysis of the vaso-motor system of nerves is present, by the employment of quinine. The actions of quinine, bebeerine, and salicine may be taken as similar, their antipyretic action being due partly to their tonic effects on the vascular system, and partly to their antiseptic action on the microzymes which are supposed to be the cause of the fever. Until we know more regarding the etiology of rheumatism, it is useless to conjecture how salicine acts in that disease. It is well known that quinine is quite as efficacious in the treatment of rheumatic fever as salicine, but the latter, being a novelty, has attracted more attention. One property common to salicine, salicylic acid, and the salicylates, is that they have no power of reducing temperature in a person in a state of good health, and also, I am inclined to say that salicine and salicylic acid have no beneficial action on heart-affections when the temperature has fallen to the normal. Dr. Maclagan, of Dundee, states this latter fact regarding salicine, and I think those who have used either salicylic acid or the salicylates will bear me out in saying that these latter drugs are powerless for good when the temperature is either normal or nearly normal, as in subacute rheumatism, where the employment of these remedies is attended with almost no effect whatever. I have pointed out that salicylic acid taken internally by a healthy person in large doses, such as 2 scruples, produces no effect on the temperature and pulse, and that beyond producing constipation, catarrh of the pharynx, and giving to the breath for several days a strong smell of its solutions, it has no further action. In corroboration of this, Dr. Danewsky says "that salicylate of soda produces a very inconstant and inconsiderable influence on healthy men and animals, but in the febrile state it is most powerful." In this respect there is an analogy between this action in febrile conditions and the action of ergot on the pregnant uterus, or digitalis on the dilated heart. Dr. Danewsky again states that in the febrile state there is a diminution of blood-pressure, but when salicylate of soda is given the blood-pressure rises, and promotes the elimination of caloric.

Salicylic Acid and its Congeners.—As salicylic acid is now generally employed as the salicylate of soda or potash, I made trial of these in several cases, and found that, if used for a few days, depression and vomiting invariably came on. As I had a bad case of rheumatic fever under my care early in March of

this year, it occurred to me that small doses of carbonate of ammonia combined with the acid might counteract the depression and vomiting. A week after this case of mine had been successfully treated by this combination, Dr. Ogle, in the British Medical Journal for March 17, suggested ammonia as a combination, but did not bring forward any cases. In the Medical Examiner for April 19, 1877, Dr. Barclay, of St. George's Hospital places on record a case of rheumatic fever treated successfully by the combination, in which the temperature fell three degrees after six doses of the medicine had been administered. A friend of mine in London, who has found salicylic acid valuable in rheumatism, wrote me in spring that he always combined it with the carbonate of ammonia, As the only recorded case treated in this way which I have observed in the journals is that of Dr. Barclay's in the Medical Examiner, I take the liberty of giving the notes of a case treated successfully by salicylate of ammonia. It is by the kindness of Dr. Sanderson, of Musselburgh, that I am permitted to give the following notes of a case which I had under my care while assisting him during last winter.

Case of Rheumatic Pericarditis and Endocarditis treated successfully by the Salicylate of Ammonia.—On March 9, 1877, I was called to see A. C., millworker, an anæmic girl of seventeen, who was suffering from a cough and profuse expectoration. I found that there was distinct evidence of the presence of bronchitis. and ordered an expectorant mixture, with but little benefit. The patient at this time complained but slightly of pains in the joints, and therefore the possibility of rheumatic fever did not present itself to me. On Sunday the 11th I found the patient very much worse, the breathing very much accelerated, and considerable dyspnœa. The pulse was quick and irregular, the temperature excessively high, but owing to these notes being written some time after the case was attended, I failed to keep a register of temperature and pulse. The patient was covered with a profuse acid-smelling perspiration. On examining the chest it was found that there was great swelling over the left side, and on placing the hand over the præcordium, a rasping sensation was communicated to it. On percussion, it was found that there was complete dulness in front from the clavicle above to the foot of the true ribs on the left side. On auscultation, the double-friction murmur was heard all over the cardiac region. It was now clear what was the true nature of the case. The treatment which I adopted in the first instance was the application of a blister (4 × 4) over the heart, and the internal administration of powders, consisting of calomel (1 gr.) and Dover's powder (5 grs.) every three hours. This treatment was carried on from the Sunday evening till the

Tuesday evening, the patient being at that time worse instead of better. On the Monday the breathing was very much impeded, greatly increased action of the right lung being present, due to the great quantity of effusion on the left side. On the evening of Tuesday, finding no improvement, I adopted the following formula :—

℞. Acid salicylæ, ʒ iij.; ammon. carb., ʒ iss.; aquam, ad ℥ vj. Sig. A tablespoonful every three hours.

On visiting the patient next day, I found that she had experienced great good from even the first dose of the medicine. The heart was much quieter in its action, the effusion had begun to lessen, and in a week, still continuing this treatment, the effusion had entirely disappeared, and the swelling on the left side subsided. The double frottement murmur had entirely disappeared, and in its place I now heard at the apex a distinct systolic bruit, telling of temporary mischief, at least, in the endocardial lining of the mitral valve. The patient still continued the salicylate of ammonia, and informed me that it had a most wonderfully quieting effect on her heart when its action was disturbed in any way. Having occasion, about a month after this date, to examine this patient's chest, I was delighted to find the heart-sounds entirely normal. In conclusion, I may say that no stimulants were administered during the whole course of the disease, the patient's dietary throughout consisting of milk and beef-tea.

I must now draw my remarks to a close, and in doing so, I would say that the combinations of the acid with iron and quinine have proved very successful in treating pyrexia. The latter, introduced to the notice of the profession by Dr. Graham Brown, is, according to some, the most powerful antipyretic extant.—*Edinburgh Med. Journal, Dec.* 1877, *p.* 494.

10.—USE OF TURPENTINE IN DISEASED STATES OF THE SYSTEM OF AN ACUTE CHARACTER.

By R. Persse White, Esq., Surgeon to the Meath Hospital, Dublin.

So long ago as 1860, I was led to try the use of turpentine in a case of typhoid fever in a young lady, which had run a course of twenty-eight days without extreme severity. The diarrhœa was not severe, and was kept in check by acetate of lead, with small doses of opium.

On the twenty-ninth day of the patient's illness, in fact on the first of her convalescence, the symptoms had all abated, and she seemed to have overcome her illness. On the thirtieth day, on visiting her, I found her in a state of terrible excitement. There was some cause, but not enough, to account for

her state. Fearing mischief, I at once sent for a leading physician from Dublin. He advised the use of turpentine, but his advice was based on the view that there was uræmic poisoning; for, during his visit, she had severe convulsive movements of her face. The urine was scanty; it was tested, but did not show any morbid condition. On the thirty-first day, her night was terrible, with violent raving and restlessness; no sleep. On the thirty-second day, she was in a worse state, and almost collapsed. The sphincters were failing. The turpentine mixture had been continued since the consultation, but there was no benefit from it. She died that night.

In this case, the turpentine was given for the head-symptoms. The next cases in which I used turpentine were various, and at different stages of the disease. In my earlier practice, and in hospital practice, in 1873, when I acted as physician to the Meath Hospital in the absence of my colleague Dr. Stokes, and of his colleague in the medical wards, I had at least one great case which showed the value of turpentine in typhoid fever. Here the chest was, in the latter stage of the fever, attacked with severe bronchitis, the bowels being much too free at the same time. The attack of bronchitis was intense, of the form common at that time; but after the second day she rallied, and passed on to complete recovery. I saw her in health long afterwards. Bronchitis was for some years an almost constant attendant on typhoid, and often the cause of death.

My mode of giving the turpentine was as follows. If bronchitis were present, and even if diarrhœa complicated the case, I gave what was known as my turpentine mixture. ℞. Terebinthinæ olei, ʒ ii; liquoris potassæ ʒ ii; mucilaginis acaciæ ʒ iv; syrupi papaveris albi, syrupi floris aurantii, āā ʒ viii; aquæ camphoræ q. s. ad. ℥ viii. Fiat mistura. A tablespoonful to be taken every fourth hour, the bottle being first shaken.

Since I commenced that treatment, I never lost any case of typhoid from either bronchitis or diarrhœa, or from its sequelæ of ulceration or hemorrhage. Each epidemic of typhoid (and at present there is a wide-spread and severe one passing over the country) is marked by its peculiar characters. In the present time, most of the cases of typhoid are characterised by intense pain in the abdomen, with enormous flatulent distension. Constipation exists before the illness is developed, and at times during its progress; and when it is removed by purgatives, diarrhœa sets in. I give a short sketch of some cases recently under my care.

I was called to visit a boy residing several miles from the city. The child sickened slightly about September 30th, but nothing was suspected for some days. On Monday, October

4th, I found him very ill, and I at once suspected typhoid fever. He had on Sunday some vomiting and slight diarrhœa, which was, on the 7th, followed by constipation. I found it necessary to give an enema, with turpentine. This brought away a quantity of scybala of a light colour, and gave some relief. The abdomen at this time was tumid, with some pain on pressure. I gave a small dose of castor-oil and turpentine on the succeeding day. It gave much relief, with a similar character of discharge. Between the 9th and 10th, his bowels became a little too free, but the distension of the abdomen had become so great that it pressed upon the heart and lungs, so as to give much distress. I at once ordered about ten minims of turpentine every third or fourth hour in mixture. On the 11th, there was some improvement; the pain was much less. On the 12th, he allowed me to press my hand on his abdomen, which hitherto he could not bear, and expressed himself much better. I need not detail the case. I continued the turpentine for nearly a fortnight, toward the end lessening the dose to twice each day. No diarrhæa returned, but the bowels were slightly free twice most days. After this, slight pain and diarrhœa came on, and a few doses of the turpentine were given with perfect success. This child is now quite well, running about; he convalesced in the sixth week.

The next case is that of a lady, aged 73, without any previous history of illness. On Sunday last, November 18th, she vomited, and diarrhœa quickly set in, severe in its character; blood was mixed with the stools, but in small quantity. When I first saw her next day (Monday), I put her on aromatic sulphuric acid, with small doses of Battley's liquor sedativus. This gave much relief for two days, and at my visit on Wednesday all was well; she had had only two slight motions, with no blood. In the afternoon, profuse diarrhœa occurred four times before 9 p.m.; nine times to 7 a.m. on Thursday; and five times more up to my visit, blood coming in all the later motions. I at once ordered the turpentine mixture, two tablespoonfuls every fourth hour. On Friday morning, she was going on well. The diarrhœa had ceased. There had been only two small motions since the turpentine was commenced. I saw her at night. She still continued in comfort; no more motions since morning. On Monday, November 26th, she continued in comfort; no pain and no diarrhœa; only two small motions in the twenty-four hours. On December 5th, after four days of complete constipation, she had the following draught at bedtime: ℞. Olei ricini ʒ vj; tincturæ sennæ comp. ʒ i; syrupi zingiberis ʒ ij; mucilaginis acaciæ ʒ ij; spiritûs terebinthinæ ♏xxv; aquæ cinnamomi ℥ij. This case is not concluded, but all goes on most favourably.—*British Medical Journal, Dec.* 15, 1877, *p.* 846.

11.—ON SOME POINTS OF SCIENCE AND PRACTICE CONCERNING CANCER.

By John Simon, Esq., C.B., D.C.L., F.R.S., Consulting Surgeon to St. Thomas's Hospital.

Before I start, let me say that throughout my argument I shall always use the word "cancer" in its old-fashioned surgical sense, intending it to cover all the various tumours and ulcers which we familiarly class as *malignant;* and the word "tumour," which often may include "cancer," I use restrictedly, in its pathological sense, as meaning only *tumour by process of growth.*

I. The men who within our times have theorised on the Evolution of Cancer, have equally had before them for explanation certain broad facts in the natural history of the disease. They have seen that the tendency of persons to suffer cancer runs with marked excess in particular families, or, in other words, is in a great degree hereditary. They have seen that the natural course of a cancer, left to itself, is to pass into indefinitely extending processes of local destruction, which involve such flux of organic material as must sooner or later exhaust the general strength, and end the life, of the patient. They have seen that often before this course, as regards one cancer, can complete itself, other cancers are making progress in other parts of the patient's body, to the more rapid detriment of his life; or that, at any rate, after his death, other cancers, more or less advanced, will generally be found in his body. And, not least, they have seen that surgical removal of cancer, whether by knife or caustic, is in general of no effect to cure the patient; often because of the just-mentioned presence of the disease in various other parts of the body; often, also, because the disease recurs in or near the place of removal.

The older cancer-theory of our times—the theory which was in full bloom some twenty years ago, and is even now not quite extinct, interpreted those facts to about the following effect. It conceived the patient to be *ab initio* the subject of a form of general ill-health or cachexia which disposed his entire body to form cancer, just as the entire body of a person incubating small-pox is disposed to form variolous pustules. His state before the cancer showed itself was a state of general cancerous tension. When a solitary cancerous tumour (say a skirrhous breast) came under surgical notice, it was regarded but as the partial effect of a diffused cause, the outward and visible sign of a tension to which it gave partial vent; and the many cancers, when they were seen, in lymph-glands and various other organs, were but the more perfect utterance of that original dyscrasy.

In this theory there was, as we now know, a large admixture

of wrong inference. For the better theory which is now generally accepted in its stead, our profession has been mainly indebted to the staff of the Middlesex Hospital; in the first place, to Mr. Septimus Sibley's most instructive paper of pathological statistics, published in 1859, from the experience of the cancer-wards and deadhouse of the hospital, to which he had then recently been house surgeon and registrar; and afterwards to the admirable practical teachings (too soon silenced by death) of the late Mr. Charles Moore and the late Mr. Campbell De Morgan, surgeons of the hospital.

The amended theory of cancer recognises no cancerous cachexia except such general ill-health as gradually results from the progress of cancer. It appeals to the fact familiar to us all —familiar even in a degree which often in a particular point of view makes the experience painful to us, that the person who comes to consult us with a cancer, a person whom we may at once see to be doomed to death within a year or two, is often to all appearance in rude general health. The theory does not necessarily pretend to explain the origin of the local disease which in such a case is brought to our notice; but, starting from that as fact, it argues what must result from it. Given (it says) one primary tumour, all other facts of the case follow from it by logic of humoral sympathy; just as, in the story of syphilis, secondary and tertiary consequences need only the one hard chancre to account for them. The cancerous cachexia, like the syphilitic cachexia, is but an affair of *progressive infection*; essentially by the juices of the body—the lymph and blood, but sometimes also accidentally in other ways; an affair only of infection, of ever-widening infection, from the one first established focus of disease. How that first focus came to be, and how it came to have its wonderful endowment of infectiveness, are questions which must be separately argued; but meanwhile (says the theory) let us frankly recognise that, where our cancer-patients show certain general signs of disturbed health, presumably this "cachexia" is the effect, not the cause, of the cancer.

II. Of late years, too, there has been change in the point of view in which pathologists have regarded the Anatomy of Cancer. Thirty years ago, cancer was supposed to be a specific new bodily texture, having (as cartilage or muscle has) an organisation proper to itself in contrast with other textures, and proper to it in all its forms. In those early days of modern histology, not all men who had picked up a smattering of Schwann were competent to understand the real physiological significance of his doctrine: and many a microscopist of those days talked of "cancer-cells" as he talked of nerve-cells and fat-cells; professing that, by the visible presence or absence of

these characteristic cells which he described, every tumour would declare itself malignant or non-malignant. This (in the sense in which it was meant) was an absurd twist to be given to pathology; and I remember that even in 1847, in the first pathological lecture which I gave at St. Thomas's Hospital, I ventured to raise my voice against it. From across the North Sea, however, there was then happily beginning to be heard a voice far stronger than mine; and Virchow, rapidly laying the foundations of his now well-known system of textural pathology, soon consigned to the limbo of vanity those mare's-nest "cancer-cells" of the too easily satisfied preceding decennium. The profounder and permanent work which since that time has been done in the anatomy of cancerous and other tumours is of really immense amount—immense, even if we regard only the contributions which have been made to it in the German language; but even yet it is far from complete, and the generalisations to which at present it seems to point must of course be deemed subject to correction by further contributions as they come in.

It is impossible that on this occasion I should attempt to do justice in detail to even any of the more finished sections of that immense anatomical labour; and I will only venture to describe in a few sentences what, up to the present time, seems to me their essential outcome. It seems that cancers have not, as was pretended, any one structure common to them all; that on the contrary, different species of cancer have structures as dissimilar as the structures of bone and muscle. One principle of similarity does, indeed, apply to them all; not the principle of likeness *per capita*, but the principle of likeness *per stripes*. Each primary tumour has characters impressed on it, and for the most part very emphatically impressed, by what we may call its particular local parentage. The different species represent different textural origins; each texture which starts a primary cancer having, so to speak, a cancer proper to itself. Mucous and cutaneous surfaces and involutions, connective tissues, pigment-tissues, bone and periosteum, muscle-substance. lymph-gland, nerve-substance, and so forth: each has its own distinctive way or ways of growing primary cancer; and as we study the whole range of cancerous tumours, from skirrhus to glioma, we seem to see that the growth of each makes itself only gradually divergent from the normal growth-type of the texture which it represents. And as each sort of primary cancer expresses in this way more or less clearly the organ which started it, so, of course, it is in intimate structural affinity with the non-infective tumours of the same organ; and I believe that the best histologists, when they contemplate the first textural beginnings of a cancer in any affected organ, see

only such simple signs of textural overgrowth as might equally be the beginnings of a non-infective tumour.

In a certain sense, however, though a sense widely different from that of the doctrine of thirty years ago, we may still say that the various sorts of cancer have morphological characters in common; but the likenesses to which I here refer are likenesses rather of expression than of feature. Thus, for instance, it seems general to cancers that the overgrowing textural elements of which they primarily consist do not develop into ripe texture, but remain more or less immature; and that in some cases they exhibit a marked reversion to very early embryonic type. It seems also general to cancerous, as compared with non-cancerous tumours of respectively the same textural parentage, that, as they grow, their first textural type soon becomes obscured: on the one hand, by the crowding of forms which, in proportion as the process is vehement, will more and more be immature or embryonic; and on the other hand by evidences, which are sometimes extreme, of the tendency of the new growth to degeneration. On the whole, then, the knowledge which anatomy hitherto contributes to the explanation of cancer is but indirect, and rather negative than positive in its bearings. The anatomical forms explain nothing in regard of the property of infectiousness which is associated with them, and which, as I will hereafter show, constitutes the real puzzle of the disease. The anatomical forms are matters of mere local accident; but the infectiousness of the cancer represents its very cause.

III. Of all the Etiological characters of Cancer, that which I think incomparably the most important is the property of infectiousness which its cause imparts to it, and which in fact makes the identity of the disease.

I need not say that in a great variety of diseases which are primarily local (including many which we can ourselves start by inoculation for purposes of study) the primarily diseased part is able to infect other parts by means of the lymph and blood which it directly or indirectly transmits to them, and into which it has shed its contagium; and, in the case of cancer, evidence has long existed that those are the essential means by which the disease, when once started at any spot, tends to produce secondary and tertiary cancers in other parts of the body. Molecules, larger or smaller, of the primary cancer are always apt to be contained in the outflowing lymph and blood; and, in particular cases, the growth intrudes into veins with masses which are visible to the naked eye, and bits which detach themselves pass on with the blood till they become fixed as embola in smaller vessels.

Secondary and tertiary cancers, beginning to show them-

selves in the organs which have been infected from the primary seat of the disease, invariably imitate in their structure the particular structure (whatever it was) of the primary cancer—the epithelioma, the glioma, the fibro-sarcoma, or what not; and commonly they imitate it with an exactness which extends even to its minute individual peculiarities. When this remarkable fact is taken in connection with that other (which I just before mentioned) of the frequent passage of shaped texture-elements from the primary cancer into the lymph and blood, the simple theory at once suggests itself, that secondary and tertiary cancers are the outcome of a kind of natural budding-process from the primary; that living cancer-elements, floated from the one place to the other, and carrying with them a strong germ-power of their own, affix themselves as parasites to the textures which they reach, and grow there to an unlimited extent, in forms which (from the nature of the case) repeat exactly, as would vegetable grafts, the features of their parent stock. This explanation of the secondary and tertiary growths in cancer would seem to have some warrant from its simplicity; but though, as matter of fact, it seems certain that in some cases transported bits of cancer-tissue do really engraft themselves in new sites in the manner which the theory describes, such graftings appear to be quite exceptional, and their result, as regards the growth of the grafts, is questionable.

The process in which the secondary and tertiary growths in general arise is apparently of a zymotic kind; and certain observations relating to it which have been made by Dr. Creighton seem to me of quite singular interest with regard to the genesiology of cancer. In the course, namely of some elaborate cancer-studies which were made by him under the Lords of the Council—studies which it was my great good fortune to be able officially to promote, Dr. Creighton made much minute investigation of secondary cancer-nodules in liver and lymph-glands, and of various other secondary and tertiary cancers; and the explanation, which this research has seemed firmly to establish, of the meaning of such consecutive nodules is, not that they represent the primary disease propagating itself by offshoots to new parts, but that they are autochthonic growths of the parts where they occur. It appears that, under the contact-influence of matter from the primary cancer, the textural elements of the next affected organ pass, by successive changes of their own, into growth of a new sort, by which, as it advances, the secondary nodules are gradually evolved into their wonderfully close textural imitation of the distinctive texture of the primary disease. Almost invariably this imitative growth seems to be not in any degree modified by the

anatomical type of the organ in which it occurs: Dr. Creighton's single (but only partial) exception being the case of the ovary; which organ, when secondarily cancerised, seems able to add more or less cyst formation on its own account to whatever cancer-type it has got by contagion.

The spreading of cancer by such a process as Dr. Creighton describes must certainly be regarded as one of the most curious of all hitherto observed facts of contagion. Pause and consider what it signifies. The primary cancer, anatomically regarded, is a definite original texture of the body, growing a modified process of growth, in which, notwithstanding its modification, the original type of the texture can be identified; and now this modified texture is seen to possess the marvellous endowment, that, coming into inoculative relations with other textures of the same body, it compels those second textures to abandon their own textural identity, and heterologise themselves to the textural pattern of the tumour. Think how, as we watch in different cases the workings of the contagium of cancer, we see the unity of that principle expressed in infinite variety of results—see the skirrhus of breast, or epithelioma of lip or tongue, counterfeited by the textural elements of the infected lymph-gland,—see some melanotic sarcoma of the chorioid, or some follicular form of bowel-cancer, reproduced by the cells of the infected liver,—see the infected lung representing some osteo-sarcoma of the femur or some cysto-chondroma of testicle! And as we contemplate, in the light of Dr. Creighton's observations, the local progress of the primary disease (say some glioma of the retina, or some round-celled sarcoma in a limb), we become, I think, better able to understand the meaning of that singular so-called "infiltrativeness" which it possesses, as compared with the merely displacing power of (say) a large fatty tumour or a large collection of hydatids: an "infiltrativeness" with which it seems to abolish, but not by stretching, the various barrier-surfaces against which it comes; an "infiltrativeness" which, it would seem, may be nothing else than the circumferential contagious working of the cancer on the elements of each texture which it reaches.

I need hardly say that the ultimate meaning of these strange phenomena is beyond our present powers of explanation. Dr. Creighton's facts tend irresistibly to remind one of the molecular changes in sexual impregnation. As one sees the emigrant forms of the primary cancer melt away in the lymph-gland to which they have been borne, and then the texture of that lymph-gland begin a series of developmental changes which will eventuate in a new presentation of the parent disease, one's mind recalls the original working of the spermatic force which

called the whole organism into being, and one is tempted to speculate whether, perhaps, the essential power of the "malignant disease," its power of specific fertilisation, may really be that the part has in activity in it (under unexplained conditions) some normal or abnormal survival of that ever-marvellous first ferment. But, if there are points of view in which that sort of speculation might seem to find encouragement from facts (and perhaps especially as regards the more fungific cancers of early life) other points of view, especially as regards skirrhus and the other epitheliomata, seem to me to suggest a widely different, though not necessarily incompatible, line of speculation. As our patient in extremity of advanced syphilitic poisoning, with tertiary gummatous tumours widely diffused among the organs of his body, tells us of the little chancre-inoculation ten years ago from which this general-tumour formation has resulted; or as we call to mind the equally demonstrable contagium of tuberculosis, and the profuse (though only miliary) tumour-formation which this contagium specifically promotes,—can we, with those cases before us, feel sure that no analogous exterior influence, nothing of the nature of a morbid poison, is concerned in the causation of cancer?

Our present ætiological position seems, in short, to be this. In the genesis of the primary cancer, we have evidence of two forces: one, the natural growth-power of the texture, the other a power which is at least relatively foreign; and the cancer, which will act zymotically on other organs, expresses the co-operation of those two powers. Whether the process, as regards its unknown factor, depends, directly or indirectly, on some contagium from the outer world, or is from first to last merely the abnormal play of forces native to the body, is the question which waits to be solved. In our present imperfect state of knowledge with regard to many of the requisite elements for judgment, it would, I think, be unwise to attribute impossibility to either of the alternatives. In relation to them both, as well as to other conceivable hypotheses of cancer, our scientific need and duty is to continue observing, as accurately as we can, every local and personal and hereditary condition which may seem to act, either attractively or repellingly, on the factor which it is our aim to understand; and of such indications there are already some which I think valuable. Thus, for instance, the fact (above noted) that cancer has marked affinity for organs which are already in certain accidental ways disordered, seems to show that the unknown exciting cause either is not native to the body, or at least is not specially an attribute of the texture in which the disease breaks out. And the fact (as it appears to be) that cancer, though eminently contagious from part to part in the affected

body, can hardly, if at all, be communicated to any other body, even among animals of the same sort, by artificial inoculations, injections and transplantations, seems to say—first, that the unknown factor in cancer can only operate where certain general predisposing conditions exist, — and, secondly, that cancer is perhaps not an hereditary disease, except as regards those predisposing conditions.

IV. In coming now to the Treatment of Cancer, I come to what I cannot but describe as hitherto matter for most painful contemplation. We practically have no treatment of cancer (in the sense of curative or preventive treatment) except such as consists in endeavours, in selected cases, to extirpate it with knife or caustic. In a very large majority of cancer-cases, probably more than three-fourths of the entire number, there can hardly be any serious thought of recourse to this one expedient; sometimes because of the original locality and perhaps visceral relations of the disease; sometimes because the cancer, since its origin, has made too much progress; and sometimes because of conditions concerning the patient's general health. To knife or caustic, the sole present resource of our art, we, therefore, can only resort in favour of the much smaller proportion (probably not as much as one-quarter) of our cases. And, in regard of this favoured minority, what is the good which surgery can promise? First, it can promise a microscopical hope—a hope which, on the whole, is so small as to be scarcely distinguishable from despair, that the disease will be radically cured by the operation. Secondly, it can hold out hopes, the exact nature and the strength of which will differ very greatly in different cases, but which, at their very best, are only hopes of palliation: sometimes the prospect that, under circumstances which otherwise threaten very speedy death, immediate, though only brief, respite will be obtained; sometimes the possibility (more or less) that such real check will be given to the disease as may sensibly affect the duration and (for longer or shorter time) the comfort of life; sometimes the object that particular local horrors of the disease will, if even only for a very short time, be abated. All this, taken at its best, is but poor measure of comfort for us to be able to give in respect of a disease so frequent and so dreadful as cancer. And even as much as this for but one-quarter of the cases!

We have the pain of knowing that even the eminent authorities whose names are most identified with the advocacy of early and (in cases of relapse) repeated operations express almost no hope of radically curing the disease by such treatment. Mr. Moore, when last discussing in print (1870) the grounds on which, in his opinion, operations for cancer might be advised, expressly said that the notion of eradicating the disease by operation can

but rarely sway the mind of surgeon or patient; for that, though there have been instances in which cancer of the lip, and more rarely of the breast, removed by operation, has not, even after many years, reappeared, such cases, on account of their rarity, can have but little influence in the decision. Mr. De Morgan, again, in the famous discussion on cancer of 1874, spoke of the "all but certainty of the disease's recurrence, remove it as we will." And this phrase of his drew an instructive comment from Sir James Paget; "I do not know," said Sir James, "what percentage *almost* means; but I will venture to say, speaking of ordinary typical cancer of the breast, or any other part which is its most frequent seat, that the number of cases in which it does not recur is not more than one in five hundred."—*British Medical Journal, Feb.* 16, 1878, *p.* 219.

DISEASES OF THE NERVOUS SYSTEM.

12.—ON THE PATHOLOGY OF TETANUS.

By the EDITOR OF THE LANCET.

Of the large number of hypotheses which have been entertained to explain the phenomena of tetanus, one of the most important is that which we may call the humoral or septic. According to this the disease is induced by the entrance into the blood of a poison which is generated (in traumatic tetanus) in the wound, and acts by a special elective affinity on the spinal cord and medulla, perhaps also on the motor and other centres of the brain. The generation of this poison is held to depend either on conditions of the wound itself, on that of the system in general, or on external causes of a septic nature, or having power to produce septic changes. The emotional and moral conditions which seem in some cases to favour it, the alternation of heat and cold, and the presence of foreign bodies of an irritating nature in the wound, are regarded as so many stimuli to the production of this poison. Of its nature many views are held, some believing that it is specially connected with the decomposition of nerve-tissue, and others that it is analogous to the poison of pyæmia. Most believe that it enters into the blood circulation, but there have not been wanting those who have held, following the old theory of Carus, that there existed a nerve circulation, and that the poison was actually transmitted by the nerves themselves—a view hypothetically resuscitated by Mr. Howse. But of these and other phases of the septic theory there remains the fact that they are all hypothetical—that, if true, they are not proved, nor even certainly indicated by facts. But if the poison does thus, by

the medium of the vessels or lymphatics or otherwise, reach the nerve-centres, in what way does it act upon them? Here, again, a variety of theories suggest themselves. Does the poison act directly on the centres, either as an irritant producing nervous discharge, or as a mere excitant setting up a condition of excessive reflex excitability, or does it act on the bloodvessels first, chiefly in certain regions, and induce changes in the nerve-cells as a secondary result? The latter view is supported by Dr. Coats on the ground of his microscopic observations, and by Dr. Dickinson on similar and other grounds.

Before we consider the facts which tell for or against this theory, we may briefly advert to the most important of the other hypotheses—that, namely, of nerve irritation. According to this, the irritation of a peripheral sensory nerve-filament, or mixed nerve-trunk, produces the condition in the nerve-centres of the medulla and cord which leads to the disease, and this condition depends on an exaggerated reflex excitability or direct reflex excitation. At the present moment we shall not inquire what are the centres affected, or as to the exact mode of affection; for whether we adopt the view of paralysis of the cerebral centre of control of reflex action of Setschenoff, or Dr. Ringer's ingenious theory of lessened resistance, or the idea of excessive irritability of bulbo-spinal centres, or, as some have suggested, of sympathetic ganglionic centres, the starting-point of the stimulation may be the same. This question may best be reserved till we have considered the symptomatic phenomena of the disease.

It is a curious fact that the two schools of thought—the humoral and the nervous,—by whatever minor differences of detail they may be separated amongst themselves, appear to number on either side a majority either of physiologists or pathological histologists, the latter especially inclining to the humoral theory, whilst the former are, with few exceptions, in favour of the theory of nervous irritation. In addition to those pathologists whom we have mentioned, Billroth's name is conspicuous as a champion of the humoral theory, whilst on the other side are the names of Marshall Hall, Brown-Séquard, Volkmann, Bernard, Vulpian, Weber, and many others.

The pathological anatomy of tetanus has been very fully worked out with regard to the spinal cord and the peripheral nerves in the neighbourhood of the wound, and the changes in these may be very briefly summarised. In a considerable number of recorded cases there has been found some excessive vascularity of a nerve-trunk, or various conditions of inflammation, from slight hyperæmia and exudation to actual suppuration, extending upwards for some distance; and, in some few of these, an irritant body has been found in direct relation to

the nerve. In some very few cases the inflammatory condition has been found to extend as far as the cord. The cord itself with the naked eye has, in many cases, been observed to be normal, in other instances small hemorrhages occurring either along the vessels or in its substance. Microscopical examination, as shown by the researches of Lockhart Clarke, Dickinson, Allbutt, Michaud, and many others, usually discovers changes in connexion with bloodvessels, consisting in engorgement, hemorrhagic or fibrinous extravasation, exudation of leucocytes, and more or less granular disintegration of the nerve-tissue, with also, as was especially noted by Michaud, a marked proliferation and exudation around the central canal, and in the grey commissure. Dr. Lockhart Clarke, who was, we believe, the first to study these changes systematically, regarded them as due to a morbid state of the bloodvessels, but he showed also that they could not be regarded either as the cause of the spasms, or as produced by the functional excitement. And, so far as we know, no one has materially extended his observations, or been able to elicit from microscopic examination any further clue to the pathology of the disease.

It will be needful to return to this subject, so we shall at present only briefly indicate the chief arguments which make for or against the two great hypotheses. In favour of the septic theory may be urged the analogy of the results of some poisons, notably strychnia and the poison of hydrophobia; the evident effects on the bloodvessels, as seen with the microscope; the difficulties in explaining localisation on the nervous theory; the occasional endemicity of tetanus, or its concurrence in several cases together; its reputed production by wounds with arrows poisoned with putrid flesh; and the dependence of trismus nascentium on insanitary conditions. Against it are urged the great rapidity of onset and death in some cases where no decomposition can be assumed; the entire failure to inoculate the disease, or to produce it, as in Arloing and Tripier's experiments, by injection of a large quantity of blood from an animal with tetanus into a healthy one; and, on the other hand, the facts of direct experimental production of tetanus by nerve irritation, and the cure of tetanus by removal of an irritant body or by nerve section, which could not influence the action of a poison already formed.

Against the theory of nerve-irritation are the difficulties due to the peculiar localisation of the reflex spasm, its commencement in relation to centres far distant from the immediate origin of the nerve—often, indeed, the region of the irritated nerve being but little affected, and no direct reflex action induced. The absence of evident changes in the nerves, in many cases, is also thought to be opposed to this view. Moreover, though it

is well known that numerous experimenters have failed to evoke tetanus by experimental irritation of nerve-trunks, it has been shown by Weber and Heidenhain that the slow irritation of a nerve by special methods does produce all the phenomena of tetanus. These experiments are conclusive as to the possibility of nerve-irritation alone giving rise to the disease, and establish this as one, at least, of the methods of its origination. —*Lancet, Dec.* 15, 1877, *p.* 891.

13.—TETANUS TREATED BY CHLORAL AND INDIAN HEMP.

By Dr. A. P. Boon, District Medical Officer, St. Kitts, West Indies.

The treatment of tetanus has always been a subject of interest to surgeons, especially in the tropics, where the disease is comparatively frequent, where it is sometimes so prevalent that I have known surgeons refrain from operating, whenever it was possible, until the risk of the disease became less. It is well known that there is probably some connexion between tetanus and atmospheric disturbances, and I could, if it came within the scope of this article, adduce several interesting cases in support of this theory. It is admitted that, however much we may be in the dark as to the pathology of tetanus, we know that the mischief in the nervous system tends to right itself; it therefore seems to me that the main object of the practical surgeon, bearing this in mind, is to gain time, to keep death at arm's length; for every day the patient gets over improves his chances, and is a step towards recovery. Now, cases of tetanus may die of exhaustion, but rarely; I have never seen such a termination; they usually die suddenly of apnœa, from spasm of the muscles of respiration and of the glottis. What we have to do, then, is to keep up the patient's strength, and lessen, by all the means in our power, the severity and frequency of the tetanic spasms.

Four of the last five cases that have come under my notice having terminated in recovery, three of which were treated by myself, and two by my father, I think it worth while to record the plan of treatment; there is little, if anything, novel in it, but I venture to think that, if carried out carefully and thoroughly, it will be successful in a fair proportion of cases.

The cases are as under :—

Case 1.—Traumatic tetanus, in a Portuguese boy, aged ten years, coming on thirty-six hours after a wound of the foot with a hoe. This case progressed favourably, the spasms becoming slight and less frequent under treatment; but on the night of the ninth day he was seized with a severe one, in which he died.

Case 2.—Traumatic tetanus in a negro, aged eighteen years, coming on forty-eight hours after a severe sprain of the muscles of the back. Recovered.

Case 3.—Traumatic tetanus in a negro, aged fifty years, coming on six days after the foot had been pierced by a crow-bar. Recovered.

Case 4.—Idiopathic tetanus in a negro boy, aged ten years. Recovered.

Case 5.—Idiopathic tetanus in a Portuguese boy, aged fourteen years, coming on forty-eight hours after plunging into a trough of cold water while heated. Recovered.

The first two and the last one were cases of acute tetanus; the third was not so severe, but could hardly be classed as chronic tetanus, for at times the spasm was intense; the fourth case was one of chronic tetanus.

The course of treatment which I endeavour to carry out is as follows:—First. The room must be dark and quiet; draughts are to be carefully excluded. Too much stress cannot be placed on this; the least rush of cold air, flash of light, or even sudden noise, may bring on a spasm. On one occasion the contact of my cold hand when feeling the pulse produced one. Another case of traumatic tetanus was progressing favourably, when suddenly in the night there was an alarm of fire; great noise and confusion ensued, windows were thrown open, and, caused either by that or by the fright severe spasms came on, in one of which he died in an hour or two.

Second. Nourishment should be given freely, in a liquid form, and at frequent intervals; it should be always warm, cold drink being avoided for the same reason that cold air is excluded. Stimulants should be administered from the first in small quantities, say four or six ounces of brandy in the twenty-four hours, and increased if the pulse indicate it. This will hardly ever be necessary; for the pulse usually keeps up to the last, death, as I before stated, being rarely from exhaustion.

Third. Never give purgatives. It is obvious that when our object is to keep the nervous system quiet we should avoid purgatives of all kinds. Imagine the amount of irritation a dose of croton oil must cause; I have seen an exacerbation of all the symptoms occur after its exhibition.

Fourth. Hydrate of chloral, together with extract of cannabis indica, is to be given in rapidly increasing doses, until the frequency and severity of the spasms are controlled. I generally commence with thirty grains of chloral in an ounce of water, and two grains of the extract of Indian hemp, in the form of pill, every three or four hours for an adult, and increase the former by fifteen grains and the latter by two grains until

the desired effect is produced, when the spasms will be few and far between, the abdominal muscles almost normally flaccid, and the mouth opened to at least an inch; the patient is then in a state of stupor from which he can be roused to take nourishment. I find that sixty grains of chloral and four grains of the extract is a full dose in fairly severe cases. It seems to me that the chloral controls the frequency of the attacks, and the hemp the intensity of the muscular contractions, and consequently the dose of either drug should be increased according to these indications. It is remarkable in some cases how the disease may be controlled in this manner. For instance, in Case 3, on the sixteenth day, as there had been no spasms for seventy-two hours, I ordered the chloral to be discontinued. A few hours afterwards, in my presence, he had a severe spasm; the chloral was at once resumed, and there was no return of the disorder until the twenty-first day, when precisely the same thing again occurred. From this it will be seen how cautious we must be in leaving off the medicines. About the end of the second week, many cases are suddenly carried off by a spasm when they are thought to be out of danger. I am convinced that this happened in Case 1, and that the nurse, imagining her patient safe, relaxed her watchfulness. From what I have seen I am inclined to lessen the frequency of the doses rather than their quantity when the patient gets over the second week, and this must be done with the utmost caution.

I do not claim for this treatment that it will be invariably successful. There are cases of tetanus, as there are cases of any other acute disease, that run their course to a fatal termination, apparently defying all attempts at treatment; but I think the results of the above cases are encouraging, and hope on another occasion to record other successful cases treated after the manner I have indicated.

While writing the above I have come across, in a pamphlet on tetanus, published in 1856, by Dr. Jackson, late Presidency surgeon at Calcutta, the following passage, which singularly bears me out in the view I have taken of the treatment of tetanus. After stating that he had tried all methods of treatment without success, he goes on to say: "Ice to the spine, opium, camphor, I have tried, but I have no faith in them. I have found hemp and aloes much more beneficial, and from the use of chloroform I have seen patients derive great relief; but it was not until I had used this in combination with hemp and aloes, supporting the patient with good diet, quinine, and wine, and keeping him in a state of perfect rest, that I have met with anything like success."

I have already mentioned that death usually occurs in a

spasm from apnœa. Now, even when the patient is apparently dead, we must not give up. Artificial respiration should be at once commenced and persevered with for a time; we should not wait even until the rigid muscles become partially relaxed, but the moment insensibility comes on an attempt may be made to get air into the lungs. I am convinced that many deaths from tetanus may thus be averted, at least for a time, and the patient given another chance of life.—*Lancet, Feb.* 16, 1878, *p.* 230.

14.—A CASE OF IDIOPATHIC TETANUS TREATED WITH ATROPIA AND CHLORAL HYDRATE.

By Dr. E. Watson Paul, Glastonbury, Somerset.

On Sept. 28th last, I was sent for to see a patient who "could not open her mouth." Such was the message I received. On arrival I found the patient (a young, unmarried woman) suffering from trismus, and spasm of the whole of the muscles of the neck, together with marked emprosthotonos. The patient's teeth were tightly clenched, and she could not speak, but was able to write a few words. There was no history of an injury; she referred her attack to having got her feet wet some ten days back. She had been ill five days before I was summoned. The catamenia were regular. I ordered her bowels to be immediately freed by enema consisting of half an ounce of castor oil in a pint of warm water, after which ten grains of chloral hydrate were injected into the rectum, to be continued at intervals of an hour. Food (strong beef-tea and brandy in doses of from one to two ounces) was also administered by rectum.

Sept. 29th. The patient to-day is much worse; there is constant spasm of the muscles; emprosthotonos much more marked; pulse weak and fluttering; countenance anxious, and symptoms altogether denoting failure of vital powers. I injected subcutaneously two minims of the solution of sulphate of atropia, made up to ten minims with glycerine and distilled water. Ordered nutrient enemata and chloral hydrate to be continued.

30th. Patient still in a very weak state, but there was not so great exhaustion of the vital power as I was led to expect from the symptoms yesterday. The teeth are still tightly closed, and the rigidity of the muscles is very marked. The slightest sound brings on a violent paroxysm. I again injected the atropia as before.

Oct. 1st. The patient seems a trifle better; the bowels were opened freely yesterday evening, and, the pulse, if anything, is stronger. I injected the atropia as before, and ordered continuation of nutriment by the rectum.

2nd. Slightly improved; the spasms are not so frequent, and the tongue can be protruded to the extent of an eighth of an inch between the teeth. The patient's friends, on their own responsibility, attempted to administer food by the mouth, and a violent paroxysm was the result. As the nutrient enemata have never been rejected, I ordered their continuance, also the chloral, and I again subcutaneously injected the atropia, this time increasing the dose to four minims of the solution.

3rd. Patient's bowels not having been freed since Saturday, I ordered an enema as before. This had the desired effect. The tongue can still be protruded a short distance between the teeth, the pulse is firmer, and the paroxysms not so frequent. Attributing this improvement to the subcutaneous injections, I again resorted to them as on the previous day.

4th. Patient still improving slowly but surely, but the spasms are still frequent, though not so severe. Treatment as before.

5th. Decided improvement; patient can speak indistinctly; emprosthotonos subsiding; spasms not so frequent; pulse better. Did not inject the atropia to-day, but ordered combination of chloral hydrate.

6th. Patient still improving. Decided to let her try a little food by the mouth, but without success. Injected five minims of solution of sulphate of atropia. Bowels naturally relieved to-day.

7th. Patient improving slowly; spasms occasionally, but not so frequent or severe.

8th. I again injected five minims of the atropia solution, and the patient expresses herself as feeling better. The tongue is very foul, and can be protruded about a quarter of an inch.

10th. Bowels freely relieved naturally; spasms fewer and less frequent; patient swallowed a little food by the mouth.

11th. Improvement gradual, but sure. Discontinued the injections from this date, but continued the chloral hydrate.

12th. No spasm for twenty-four hours; tongue cleaning; food entirely administered by the mouth.

The patient got up for the first time on the 15th, and on the following day was convalescent.—*Lancet, Feb.* 16, 1878, *p.* 231.

15.—ON SOME AFFECTIONS OF THE NERVOUS SYSTEM DEPENDENT UPON A GOUTY HABIT.

By Dr. J. Russell Reynolds, F.R.S., Professor of Medicine in University College, and Physician to the Hospital.

The dependence of "nervous derangement" upon a "gouty habit" has long been known; but I do not think that the frequency of such association has been fully recognised; and my object in writing this paper is to recall attention to the

subject, and to point out, so far as I am able, the characters of disturbances in the "nervous" functions which would lead to a diagnosis or suspicion of a "gouty" diathesis.

First, let me say a word or two as to both gout and "gouty habit." The former means a "special" change, of inflammatory sort, in the tissues of the joints, accompanied by the deposit in those tissues of urate of soda. The latter, the "gouty habit," means the underlying cause of those special symptoms in the joints, a something which may express itself also in various organs and in diverse ways.

We do not know what is the starting point or essential fact of "gouty habit," but this we may remember, that between the simply chemical process of food-digestion in the stomach, and the ultimate making up, and breaking down, and carrying away of the waste, of tissues—be they in brain, or nerve, or heart—there comes in the process of "assimilation"—or "concoction of the juices," as our forefathers called it—and also the conveyance of "excretory" material to excreting organs, and that these involve an infinity of changes in the quality of blood. This blood, which comes from food and goes to tissue, which comes from tissue and goes to excreting organs, may be healthy and lead to the formation of healthy tissue and the performance of healthy function; or it may be so deranged as to pervert the "nutrition" of certain tissues by a specific inflammatory process—"gout;" or it may disturb the "functions" of other organs by the impression which it makes upon them—"gouty habit." In other words, the "gouty habit" is a "toxæmia," chronic in its duration and multiform in its phases—a "blood-poisoning" induced *within* the system, and so far forth differing from the toxæmiæ with which we are so familiar, but which are introduced from *without*. That which has led me to believe that many so-called "nervous affections" are due to this "gouty habit" may be thus summarised. 1. The actual presence of gout in the joints of the individual at the time or at previous times. 2. The evidence of "gout" in ancestors or collateral relatives. 3. The frequent occurrence of acid eructations with chronic dyspepsia. 4. The emission of pale, limpid, acid urine, of low specific gravity, and with traces of albumen or sugar, or both. 5. The variability of symptoms, both as to kind and place. 6. The presence of some alterations in skin-nutrition, such as eczema and psoriasis. 7. The impossibility of referring the symptoms to any known disease of brain or spinal cord. 8. The immediate relief of such symptoms after treatment by colchicum and saline aperients, although simple purgation and treatment upon many other principles had failed.

In the endeavour to arrange this subject, there is great diffi-

culty to be encountered; but I will adopt the method of describing "groups of symptoms" under five headings.

I. *Mental Disturbances.*—Many cases have come before me in which there was great restlessness; the patient could not be still for a moment; was alternately excited and depressed; slept badly, or not at all; was intensely hysterical; and could not attend to business; while others have complained of failing memory; of want of power of attention: of suicidal thoughts; of intense melancholy; others of sounds in the ears; voices, sometimes distinct, sometimes not; and some or all of these of long continuance; but yet all disappearing under treatment upon the hypothesis I have mentioned. These symptoms often alternate with, or accompany, those which I mention next.

II. *Pain in the Head.*—Some of the most intense head-pain that I have met with has been of this character, and been relieved by treatment of an anti-gouty description. The special features are pain on one side of the head, usually parietal or occipital; "grinding" habitually; but forced into almost intolerable severity by movement, such as the jar of carriage-riding, or running down the stairs of a house; and this without any oversensitive nerve points; without tenderness o scalp; and without any aggravation by mental exertion. It is not affected by posture or by food; it is relieved by physical rest, and may disappear entirely after treatment of the kind that I have mentioned. It is not anæmic, nor neuralgic, nor dyspeptic (in the ordinary sense of that word), and it yields to nothing in the way of treatment that may be directed against those common varieties of headaches. It is very often associated with some of the other symptoms that I have mentioned, and they must be taken into account when making a diagnosis of the malady.

III. *Modified Sensations.*—1. Of these, vertigo is one of the most common, and it may exist alone. It takes sometimes the form of objective movement, but more frequently that of subjective movement, such as the sense of "swimming" or "floating" away. This vertiginous sensation is sometimes determined by posture, and occurs only when the patient lies on one side, it may be the left or the right; the apparent movement of external objects being from that side towards the other.

2. With vertigo is often associated "noise in the ears," not the sound of "voices," but drumming, hissing, singing sounds, recognised to be in the ears, or in one ear, or in the head, and not appearing to come from outside. There is not, or need not be, any mental delusion with regard to these; the patient knowing well that they are inside his organism.

3. Associated with such vertigo and tinnitus there is frequently

deafness, and the feeling of "beating in the ear"; and the symptoms are like those described by Menière; but I have found them in the vast majority of instances associated with a gouty habit. With vertigo and tinnitus there may be much mental depression, or attacks of bewilderment, amounting sometimes to those of *le petit mal*.

4. Modified sensations in the limbs may occur. A large number of people complain of "numbness," "tingling," "creeping," "deadness," or some other altered state of sensibility in the limbs, which, sometimes taking a paraplegic, sometimes a hemiplegic, distribution, have caused much anxiety; and the more so, because the suggestion of organic disease of brain or spinal cord has sometimes been conveyed, and yet all these troubles pass away. That which I have observed to be in them the most characteristic of their gouty origin is their variability in kind and locality. To-day, for example, there is "coldness" in the left leg; to-morrow "a sense of heat;" last week, a "pricking" in the right hand; the week before a "stinging" feeling on the side of the head, or in the tongue. This wide distribution and variability, so alarming to the patient, is much less alarming to the physician, who recognises in these very facts the elements for a favourable prognosis.

Here, too, I must mention the great frequency with which pains, flying pains, darting pains, often like those of ataxy, are met with in the limbs. So-called "sciatica" is of frequent occurrence, and "pleurodynia," and "myodynia" of all localities are common enough. The sciatica of gouty sort is often double, shifting from side to side with a frequency that does not improve the temper of the gouty patient, but may raise the hope of his physician as to the probability of cure. Other seats of pain are most frequently the insertion of the deltoid muscle and the inner aspect of the upper arm, the ankles, the heels, and the interscapular region. The lower mammary region on the left side is often the seat of pain, as it, indeed, is in many other maladies.

IV. *Modifications of Muscular Action.*—1. Cardiac palpitation, intermittence or irregularity of pulse, or painful aortic pulsation at and below the epigastrium, often suggest to the patient the presence of cardiac disease; and it is worthy of remark that, on the one hand, a very great amount of discomfort may often be felt by the patient when the physician can discover no change in sound of heart or rhythm of pulse; and that, on the other, disease of aortic valves, and other obvious signs of cardiac change, may often be discovered by the physician in a gouty patient, he having never been conscious of any thoracic trouble.

2. Flickering contractions of muscles in the limbs; tonic

spasm, with cramp-like pain, and "startings" on falling asleep have often appeared to me to be of gouty origin, and that for the reasons that I have assigned. Priapism, without erotic feeling, is also very common. It sometimes disturbs the sleep, is felt on awaking, but quickly disappears without emission.

3. Local weakness of muscles, such as ptosis, single or double; want of co-ordination of movement of the limbs, both upper and lower, giving an awkwardness of movement and an ataxic gait—are among the symptoms that may have the course and history that I have suggested. I have recently seen several cases of ataxia, and one with marked double ptosis, which had been treated unsuccessfully upon a syphilitic hypothesis, but which recovered speedily when the treatment was based upon a gouty theory.

V. Lastly, there are symptoms beyond those which I have mentioned, and which do not form part of the matter for my description now, but which I will simply enumerate as being further guides or helps in the diagnosis of gouty cases: 1. Dyspepsia, cardialgia, distension of stomach and colon with flatus, pyrosis, and acid eructations; 2. Varicosity of veins, with tendency, upon slight injuries, to occlusion of veins; 3. Brittleness and vertical lining of the nails of both fingers and toes; 4. Slight conjunctivitis with occasional chemosis.

The groups of symptoms that I have enumerated rather than described sometimes co-exist, sometimes alternate, and their phases are often very puzzling. They present great difficulties in diagnosis and in treatment until the clue is caught. It is often saddening to look through the carefully cherished prescriptions, and especially when they are one's own, and see the long array of drugs that have done no good—iodine, bromine, strychnine, quinine, zinc, iron, silver, cerium, arsenic, valerian, and hops, to say nothing of mercury, bitter infusions, mineral acids, and the like; but then one's sorrow may often be turned into joy—and a joy in which the patient most heartily participates—when a simple treatment, such as I have suggested, is adopted, and all the troubles disappear with a rapidity that seems quite magical, and reminds one of that beautiful process of clearing a photographic picture by cyanide of potassium. —*British Medical Journal, Dec.* 15, 1877, *p.* 842.

16.—SALICIN IN THE TREATMENT OF NEURALGIA.

By Dr. T. J. MACLAGAN, Physician to the Dundee Royal Infirmary.

The cases of neuralgia in which I have found salicin of most service, are those in which the pain comes in periodic exacerbations, and in which quinine either fails to do good, or

is for some reason inadmissible. It is, indeed, as an occasional substitute for quinine that salicin finds its place in the therapeutics of neuralgia.

Like every other remedy it is not infallible; but it has also its own sphere of usefulness, as the details of a few cases will show:—

Case 1.—A married lady, aged twenty-five, of not very robust health, occasionally suffers from attacks of facial neuralgia. I saw her thus suffering in May, 1875. On the fourth of that month, after slight exposure to cold, she was seized with pain in the right eye, and right side of forehead; the pain came on between seven and eight in the evening, and was very severe till one or two in the morning, when it left, and she fell asleep, waking about eight o'clock quite free from pain.

In the evening it returned about the same hour as on the previous day, lasted for the same time, allowing her about two o clock to fall into a sleep from which she awoke feeling quite well. On the 6th the same thing happened again. On the morning of the 7th she consulted me.

I prescribed twenty grains of quinine a day in divided doses—five grains in the morning, five in the afternoon, and ten at six in the evening. The first dose, taken at eleven, was followed by headache and troublesome tinnitus; the second, taken at two, caused intense headache and made her so deaf that she got alarmed, and did not take the evening dose. The pain returned about the usual time, and ran the same course as before. She thought it was less severe, but the headache was so bad that she could not be sure.

On the 8th she had a saline aperient which acted freely. The pain came back as before.

On the 9th she took salicin, twenty grains at eleven, twenty at two, and forty at six o'clock, in all eighty grains. She had no pain that evening; and went to bed at ten o'clock feeling quite well.

On the 10th she took the same dose, and remained free from pain.

She had no return of it; but as a precautionary measure took twenty grains of salicin three times a day for a week.

Since that time this lady has had several returns of her neuralgic pain. It has always speedily yielded to the salicin.

On the last occasion on which it returned, in June 1877, I gave instead of salicin, salicylate of soda in the same dose as the salicin, twenty, twenty and forty grains.

The pain was the same in seat and character as in former attacks, coming in the evening and vanishing at early morn. The salicylate was thus taken for two days without effect: the pain was as bad as ever. On the third day I increased the dose

to thirty, thirty and sixty grains; but the pain came back in the evening as usual; and the patient complained of the head feeling so full and heavy that she could scarcely raise it from the pillow. Had in all 280 grains of salicylate.

On the following day I stopped the salicylate, and gave salicin in the old dose, and with the same effect as before. There was no return of the pain after eighty grains had been taken. As on former occasions she took the salicin for a week. She has had no pain since then.

The salicin did not produce the least discomfort.

Case 2.—A lady, aged 45, at various times suffered severely from facial neuralgia. Being the wife of a medical man she had every justice done to her in the way of treatment.

In June, 1876, I saw her suffering acutely from pain in the course of the left supraorbital nerve. The pain was periodic, coming on about five in the morning, and lasting generally till noon. She was taking tonics to improve her general condition.

I recommended quinine in large doses, but her husband told me she could not take it, as it simply maddened her. She herself said she would rather have the pain than take quinine.

I, therefore, suggested salicin. She took twenty grains at six o'clock, and forty grains when going to bed at eleven o'clock. She slept soundly all night, and woke the next morning free from pain.

She continued to take the remedy for a week, and had no return of the pain.

She had no discomfort from the salicin.

Case 3.—A gentleman, aged 40, had an empyema, the matter of which got exit through the lung more than a year before I saw him. The chest was still in a very unsatisfactory state. His strength was, of course, much reduced, but he could go for a drive when the weather was fine.

He occasionally suffered very severely from cervico-occipital neuralgia.

I saw him thus suffering on March 6th, 1877. The pain was worse in the right occipital region, but affected also the left side. The course of both occipital nerves was tender on pressure. The pain was constant, but always got much worse in the evening.

I gave six grains of quinine every four hours during the day. He took this for three days without getting relief, but without suffering from any inconvenience except slight deafness and troublesome tinnitus. On March 10th the pain was unabated; it troubled him all day, and was, as usual, very severe in the evening.

On March 11th he got thirty grains of salicin every four hours. He had the pain that evening, but not nearly so acutely.

On the 12th (after taking 150 grains) he was free from pain, which did not return even at night. He continued to take the same dose of salicin. There was no return of the pain, but he took thirty grains of salicin twice a day for a week.

Two months afterwards the neuralgia returned. It was the same in seat and character as before. On this occasion I gave thirty grains of salicylate of soda every four hours. He took this for three days (in all 360 grains) with no relief to the pain. He complained of its destroying his appetite and making him perspire and feel very hot (though his temperature was not raised). The salicylate was omitted, and salicin was given as before, and with the same result, the pain was quite gone after six powders of thirty grains each had been taken, *i.e.*, 180 grains.

These cases suffice to show that we have in salicin a useful addition to our available means of treating a very troublesome ailment.

I have tried it in all forms of neuralgia, but have found it most serviceable in cases in which the pain is more or less periodic. It should be given in large doses. In none of my cases was a less dose than eighty grains in twenty-four hours administered. In none did it disagree or cause any symptom more troublesome than tinnitus.

A remedy which does not disagree, and which sometimes succeeds where quinine fails to relieve a periodic tic, is one which may fitly claim a place in the therapeutics of neuralgia.—*Practitioner, Nov.* 1877, *p.* 321.

17.—ON SLEEPLESSNESS AND ITS TREATMENT.

By Dr. W. Ainslie Hollis.

We may presume that very slight changes in the calibre of the brain-vessels will affect the functional relations of the organ. Increased blood-supply, though local and small in amount, may possibly produce wakefulness. The quantity of blood passing through the brain in any given unit of time depends necessarily upon two factors, the calibre of the intracranial vessels, and the rate of the blood movements.

The calibre of the cerebral vessels is determined by branches of the vaso-motor system, supplied to some extent by the upper cervical ganglion. Any cause which depressed or enfeebled these vaso-constrictors might produce wakefulness. Dr. Duckworth believes that paresis of these nerves from exhaustion is one of the causes of this distressing ailment. The wakefulness consequent on anxiety or worldly disappointment is due to this loss of vascular tone in the cerebral vessels.

Another cause of wakefulness arises from impressions made upon our organs of sense. Loud noise, a strong light, or violent

pain, may be instanced as external sense-impressions not conducive to sleep. On the other hand, a subdued recurrent sound, or the gentle shampooing of one's limbs promotes somnolence. The former possibly acts by tiring the organs of hearing, the latter by promoting an increased supply of blood elsewhere than to the brain.

Amongst the sense-impressions productive of wakefulness is a disagreeable epigastric sensation due to the flatus of dyspepsia. This symptom, with headache, and parched mouth, is a legacy bequeathed specially to the eaters of hot suppers and fashionable dinners.

A change in the velocity of the blood-stream through the vessels of the brain is a more frequent cause of wakefulness than is usually asserted. From what I have said respecting the circulation of the brain, I take it that if a slight excess of blood finds its way into the encephalic vessels, one of two results will follow: either an over-distension of some vessel locally with symptoms resultant therefrom, or an increased velocity of the blood-current generally through the brain.

The "irritable heart," combined, as it frequently is, with hypertrophy, leads to insomnia by its action on the cerebral circulation.

One of the most efficient means of inducing natural sleep is by the application of mustard poultices to the abdomen. In cases where sleeplessness arises from natural worry, abdominal flatus, or other annoyances, this remedy is invaluable. Schüler states that large sinapisms applied in this way produce first dilatation and subsequently contraction of the vessels of the pia-mater in trephined animals. They may thus act as do pediluvia and warm compresses to the abdomen, by diminishing the amount of blood in the brain. The same writer says that cold abdominal compresses and the cold-pack produce at first dilatation of these vessels, and subsequently bring about an energetic contraction of the cerebral vessels, which lasts for some hours. M. Schüler explained these changes in the supply of blood to the pia-mater as the result of a constriction or dilatation of the peripheral current-areas of the skin. The subsequent phenomena which occur with long duration of the applications, and which are exactly opposite to the initial phenomena, may be explained by the changes in the conditions of the cutaneous vessels and their consequences.

Preyer, of Jena, has advocated the administration of freshly made solution of lactate of soda for the production of sleep. About three drachms of carbonate of soda dissolved in warm water are neutralised by the addition of lactic acid. The solution may be given as a drink with sugar of milk or extract of beef. It usually induces sleep. He also states that the

administration of a quart of fresh or sour milk, or better still of sour whey, was sufficient to induce a healthy sleep. Preyer's hypothesis is that sleep may be brought on by the introduction of the fatigue products of the body. L. Meyer has to some extent confirmed these experiments.

Where the insomnia depends upon brain exhaustion, I have found (with Duckworth) that the administration of a tumbler full of hot claret and water, to which has been added sugar and nutmeg, is of great value. Both the syrup and the spice in this instance are hypnotics, according to Preyer and Cullen. The mixture must be taken just before bedtime. In slight cases of wakefulness (as we all know) the reiteration of certain word sounds mentally, at the same time drawing a slow and deep inspiration between each word, is occasionally sufficient to produce sleep.

When sleeplessness is associated with acid dyspepsia, the alkalies and alkaline earths, especially the carbonate of magnesia and bicarbonate of soda, are very useful. In cases where the indigestion is owing to a sluggish peristalsis of the stomach and upper intestines, a full dose of Gregory's powder, or ten grains of the compound rhubarb pill, will remove the disagreeable epigastric sensation and induce sleep.

The posture of the sleeper is of some importance. Many persons can sleep in their armchairs by the fireside, who court the fickle god of sleep in vain when lying upon their beds, some few hours later. The posture of the dozer and the surroundings of such a fireside nap sufficiently account for his somnolence on physiological grounds. When sleeplessness results from an overworked brain and consequent paresis of the vaso-motor nerves, the stimulus of electricity has been resorted to. Althaus recommends this treatment. Two large pads are used with a Weiss's constant battery of from ten to fifteen cells. One pad is placed over the nape of the neck, the other, which can be conveniently made of an old reflector and covered with chamois leather, is placed over the stomach. The anode is applied to the back, the kathode to the stomach for about half an hour at a time. The bromo-iodised waters of Woodhall Spa, with its balmy and invigorating air, have proved peculiarly suited to the treatment of this form of wakefulness. Entire freedom from the brain-work, which induced the symptoms in the first instance, must of course be enjoined in conjunction with one of these modes of treatment.

Impure or over-dry air will occasion sleeplessness from its irritating properties on the respiratory organs. In hot summer weather Dr. Duckworth advises the sprinkling of water over the floor of the sleeping apartment. This increases the moisture in the air, and possibly the quantity of ozone also, adding much to the comfort of the sleepers,

There are certain forms of wakefulness associated with more or less painful organic lesions, which are only to be overcome by the administration of anæsthetic or narcotic drugs. The artificial rest thus obtained differs from natural sleep in many ways, and specially in the far greater diminution of the functions of the sensory organs than does the other condition. Narcotics seem to act by directly interfering with the functional activity of the nervous system. How this cessation of function is brought about we cannot say. It is probable, however, knowing as we do how dependent is function on structure, that some actual change in the nerve stroma may temporarily occur as the results of their exhibition.

When the sleep of a patient is broken by severe pain, opium, or its alkaloid morphia, is of value, not only by directly relieving the pain, but also by assisting the production of sleep through its influence on the cerebral circulation. Opium, according to Handfield Jones, Stillé, and others, in moderate doses produces anæmia of the cerebral vessels, with a condition closely resembling sleep; in larger and in poisonous doses, there is, according to Hammond, a venous congestion of the brain-vessels due to impaired respiratory action, as it can be removed by having recourse to artificial respiration. Whilst this venous congestion lasts, profound stupor is present, and the animal is aroused with great difficulty, if at all. Great care must be taken, as we all know, in the administration of opium, that its use become not habitual to the patient. In the wakefulness due to severe neuralgia, it is frequently preferable to inject a small dose of morphia hypodermically near the branch of the nerve affected than to administer the drug by the mouth. When the want of sleep arises from the pains of muscular spasm (as for example that of gall-stone colic), or is accompanied by headache, flushing of the face, and other symptoms betokening a somewhat hyperæmic condition of the brain, it would seem that chloral hydrate was indicated. This drug has, I believe, been proved to possess considerable influence over the arterial tonus. In small doses it may possibly, as Anstie, Bouchut, and others state, increase the blood-pressure; in large doses, however, there is no doubt that both in man and in animals the arterial tension is greatly diminished. It is usually asserted that chloral diminishes the amount of blood circulating through the brain, and its hypnotic effects are ascribed to this cause. The mechanism of the cerebral circulation, as I have already noticed, would favour this idea. Any drug which relaxed the arterial tonus of the body, generally, will *a fortiori* diminish the supply of blood to the brain. For we have noticed that the dilatation of the blood-vessels of the brain is to some extent mechanically checked by the skull itself.

Accordingly less blood would probably find its way into these vessels than into others where no such impediment existed. This will be specially the case when a general dilatation of the blood-vessels occurred, as is the case in chloral poisoning.

In the wakefulness arising from defective cardiac power, on the other hand, it frequently happens that digitalis, by strengthening the force of the heart's beats, drives the blood into the capillary system more vigorously, and relieves the congestion of the central organs and the anæmia of the extremities. By thus equalising the circulation we diminish the necessity that previously existed for an increased flow of blood through the cerebral vessels, and so we promote sleep.

By many therapeutists the bromides of potassium, sodium, ammonium, and camphor are supposed to possess hypnotic properties, but my own experience with these drugs is not confirmatory of such conclusions. These salts undoubtedly act as sedatives on the nervous system, and as such may occasionally induce sleep, but they cannot, I think, be ranked as true "sleep producers."—*Practitioner, Dec.* 1877, *p.* 407.

DISEASES OF THE ORGANS OF CIRCULATION.

18.—ON THE RELIEVABLE ASPECTS OF HEART DISEASE.

By Dr. W. Moxon, Physician to, and Lecturer on Materia Medica and Therapeutics at Guy's Hospital.

All independent heart disease is hypertrophy and dilatation; and, as hypertrophy and dilatation are to a certain extent within reach of remedial means, it is worth our while to consider them carefully. Agree with me, if you please, that the true object of pathology—of clinical pathology—is to recognise and enable us to ponder upon those aspects of disease which are susceptible of relief.

"Hypertrophy" is morbid thickening of the walls of the heart, or, perhaps more properly, increase of the weight of the heart's muscle, which is not quite the same. "Dilatation" is morbid increase in the size of the cavity of the heart. You see that they are not opposites nor even alternatives of each other. In fact, you do usually meet them together. You will never see dilatation of the heart without hypertrophy of it—without, that is, increase of the weight of its muscle. But you get the heart's weight increased without increase of the size of the cavity—that is, hypertrophy without dilatation. Mind, however, if you please—and this is very important,—you must not conclude that hypertrophy of the heart exists as a sole and independent disease, like a pneumonia or an aneurism. I believe the assumption of this by authors is a partial view, and

so much so as to be an erroneous view of the subject. I do not believe in spontaneous diseased overgrowth of the muscle of the heart, any more than I believe in spontaneous diseased overgrowth of the muscle of the biceps or of the gluteus maximus. Nature does not go maliciously out of the way to disease us; nor does she sit down like an old-fashioned pathologist, and say, "Let me see, here are cavity and walls. Then we shall have —(1) cavity wider with walls unchanged; (2) cavity wider with walls thicker; (3) cavity wider with walls thinner. And again we shall have—(1) walls thicker and cavity unchanged; (2) walls thicker and cavity wider; (3) walls thicker and cavity narrower." That is how the old-fashioned pathologist, sitting at his writing-desk, followed out the affair, his very pen and ink concurring in so complete and harmonious an arrangement. But that is not Nature's plan. I believe she is sorry when disease must occur, and only allows it after resistance. To make the heart too large and strong would be gratuitous mischief. But, in fact, there is no simple hypertrophy as a disease to be combated. For if you look further, you will find that all increase of the power of the heart is compelled by some resistance that must be overcome. When you are dealing with hypertrophy in which the cavity of the heart is not enlarged, *look to the state of the urine*, and you will probably find out the cause of the hypertrophy in a morbid state of the nutrition of the textures, as revealed by their breaking into excess of urea or uric acid or oxalates. The hypertrophy is a simple index of the resistance caused to impure blood in its flow from the heart, and to fix attention on it for treatment is at best like suggesting the tying of the weathercock towards the desirable wind. The relievable aspect is in the detection and cure of the cause of the resistance.

I will put the practically important degrees of the heart's widening and thickening in a new and simple way, which I believe truly corresponds with the facts of the matter as we meet them in our patients. First, note, if you please, that any approach to exact and scientific comparison of the size of the cavity of the heart with the thickness of its walls after death is invalidated by this circumstance at least—that the proportion of the cavity to the walls depends much on whether the heart has died in systole or in diastole; and we have no means of telling whether it died in one or the other; so that the division of hypertrophy and dilatation on grounds of comparison of the size of cavity with thickness of walls after death is of no service or application during life or after death. It is not science, but pseudo-science. Its only function is to look well on paper, and to give examiners something definite to ask about in their examinations. But, in truth, you will find that

when the heart is enlarged the effects of its enlargement on the patient's comfort and life will depend upon the proportionate size of its cavity, which proportion will fall under one of these three importantly distinct conditions—conditions which produce wholly different sets of symptoms, and which must, I think, be distinguished by every one who would have clear views of this subject. They are as follows:—An enlarged heart will either (1) have no increase of the size of its cavity; or (2) its cavity will be increased, yet capable of closure during systole; or (3) its cavity will be so widened that it cannot be closed during systole. Let us consider these several conditions.

1st. When a heart is larger than it should be, but its cavity is of natural size, its possessor is in certain dangers in consequence of this condition, but *he does not feel those dangers, and his enlarged heart yields no symptoms.* This state is almost peculiar to Bright's granular kidney, and I accept it as the chief cause of the fact that seventy-five per cent. of the cerebral apoplexies with effusion of blood from bursting of the vessels, which came under my observation, were due to granular kidney. The heart's increased power is exerted in the vessels instead of being diffused on hydrostatic principles over an enlarged interior, as is the case when the cavity is widened. But the heart may reach twenty ounces in weight, and yet if its cavity is not enlarged it may give no sign at all, as we have proved on several occasions in cases where, knowing the presence of granular kidneys in persons with apoplexy, we have examined for evidences of cardiac hypertrophy and found none at all, whereas the post-mortem directly after showed hearts weighing from seventeen to twenty ounces. So much for the first condition. This is *simple hypertrophy.*

2nd. When a heart is larger than it should be, and the cavity is larger than it should be, but still capable of closure in systole, then its possessor has a distinct set of symptoms, which are those ascribed usually to "hypertrophy" in the books. Now you will generally find the symptoms of hypertrophy given in books at greater length than those of dilatation, in the proportion of about twenty pages of the one to ten pages of the oher. But although the heart, in the condition I am insisting upon, distresses its possessor by the efficiency of its power and action, yet such patients do not come before you as grave heart cases. Only exceptional organisms suffer from this condition. Very irritable systems will reveal ill effects from the change, which is calculated to produce excess and variability of vital activities. Thus, Niemeyer, following Frey, traces out the effects of a condition of the heart in which excess of blood is thrown into the arteries from the ventricle. He assumes that the excess thus in the arteries goes with a corresponding defi-

ciency in other parts of the vascular system; so that the veins are emptier than usual, and the blood runs along quickly in them through the plenty of room it finds there, whilst it goes along quickly in the arteries through the great pressure the large and efficient heart puts upon it. Thus, the whole round of the circulation is free and rapid, and vital activity is encouraged, and the symptoms of hypertrophy sanctioned by the class-books—say, as the best, Dr. Walshe's—are verified more or less along the whole list, which goes from "*a*" to "*m*" inclusive, and which sums up into floridness, bright eyes, some breathlessness on exertion, and tendency to headache. Frey's explanation is ingenious and probably right, but I think such an explanation as this could apply only to temporary over-action of the heart in such cases; for the unfilled veins, supposed in Frey's theory, would soon be filled, on account of the increased absorption which is induced by any deficiency of blood within the veins; and thus plethora and sluggishness would be reached in a day, or even in a few hours, whereas the "hypertrophy" of the heart lasts for years. I think the explanation Frey gives is a good explanation if its application be restricted to the temporary exacerbations of distress in cases of this kind, that is of hearts which have enlargement of cavity capable of complete closure in systole; but it does not apply to any very protracted and constant kind of cardiac distress, rather only to distress during exertion or in fits of palpitation. The heart in this condition gives trouble, but does not make an invalid of its possessor. We meet with it to a certain extent in football players, and in athletes generally, so take care. Such cases often come before us in life assurance practice, and raise a doubt, not easy perhaps to solve, yet often fairly to be viewed favourably to the applicant. It is to these cases Dr. Hope probably referred when he said "nearly all cases of simple hypertrophy in young men recover before forty."

What I ask you to believe in reference to these cases is, that they indicate a condition in which the cavity of the heart is larger than natural, but its walls so relatively strong that they can completely close the cavity, so that a large amount of blood is thrown into the vessels. In short, the hypertrophy of authors is more properly a state of *dilatation capable of complete systole.* But whilst a larger quantity of blood can be thrown into the vessels than is natural, it is only under excitement that the systole is thus completed. So that in periods of rest and relative depression of the heart's action, the amount of blood thrown into the vessels is about the natural quantity, and then no noticeable disturbance is occasioned. Hence the symptoms produced in these cases are variable, through times of excess and times of deficiency in the heart's action—excess causing the

usually recognised and sanctioned symptoms of hypertrophy; these symptoms, however, not being constant in any patient, but alternating with periods of quiescence and depression, which periods of quiescence and depression I have never seen noticed in descriptions of the symptoms of hypertrophy by authors on heart disease. Yet they are always present if one follows the history of any case. Now, in its periods of depression the heart does not close its widened cavities, but the distresses due to a greater degree of dilatation are not reached, because the power of the thickened heart-walls is equal to the increased labour laid upon the heart by the increased size of its cavity.

This condition of *dilatation capable of systole*, which I believe is truly the cause of the symptoms usually ascribed to simple hypertrophy, and to which Frey's explanation well applies under the restrictions just stated, is quite distinct from the simple hypertrophy of Bright's disease, which I have already described. But it is to be noticed that in some cases of Bright's disease the resistance in the vessels leads to an inability of emptying the cavities, so that the "hypertrophy" of Dr. Walshe—the second condition I am describing—is set up, and we reach thus the symptoms of dilatation capable of systole, which, when present along with albuminuria in Bright's disease, are of very evil prognosis.

3rd. On the other hand, the condition of dilatation capable of systole easily passes into the condition of dilatation incapable of systole—that is, into the condition generally recognised as "dilatation of the heart." And I shall presently draw your attention to the suddenness with which this unfortunate change is effected in many cases. A dilatation permanently incapable of systole produces cardiac cachexia and dropsy, and is the direct cause of cardiac cachexia and dropsy in all cases of these conditions. We may continue to call dilatation capable of systole by the name of "hypertrophy," and dilatation incapable of systole by the name of "dilatation." But hypertrophy and dilatation are present in both, just as whilst we call certain colours by the names yellow and red respectively, there are red and yellow in both, much yellow in bright-red, and much red in strong yellow.

The *danger* of dilatation is largely as to its suddenness; because time is required to accommodate the peripheral circulation and the cardiac hypertrophy to it. If it occur suddenly, or suddenly become more severe, the patient is apt to be at once thrown into a condition of extreme distress and peril. Such attacks of *acute dilatation of the heart* may supervene upon valvular disease. To trace, measure, and interpret these attacks is, in fact, the true problem before you in heart disease; and

the stethoscope does not enable you to do all this. Here you must be able to form a correct estimate of the size and power of the heart, and be quick in detecting the effects of dyshæmia in the several organs and systems of the body. Any valvular disease producing those murmurs, systolic or diastolic, which you rightly rejoice to perfect yourselves in discriminating, is dangerous, after all, only in proportion as it favours these attacks of cardiac dilatation, which attacks you can generally trace in the history of any case of valvular disease. To give an instance of the sudden occurrence of cardiac dilatation in valvular obstruction. A woman came into the Clinical ward exceedingly ill, and died in a week. She said she had had no rheumatic fever, and was always quite well till three weeks before admission, when she was taken at tea-time with breathlessness and faintness, from which she never quite recovered, and dropsy soon set in. Post-mortem examination showed a very closely contracted mitral valve and a very large left auricle. The valve disease was in the form of ancient fibrous thickening, and could not have actively changed four weeks before her death, nor indeed within many months of her death. So that the dilatation of the left auricle was the only discoverable cause of her sudden grave symptoms. But these attacks of sudden dilatation may come upon hearts which have no valvular disease at all. Thus, a man, aged twenty-seven, came into the Clinical ward very ill with heart disease, saying that, six weeks before admission, while in bed *in actu coitûs*, he was suddenly seized with violent dyspnœa, which never left him. He had not had rheumatism; nor, indeed, any severe illness before this attack. He showed cardiac cachexia in a marked degree; and, in spite of some relief from digitalis, he died. We then found no trace of valvular mischief, but the heart's cavities, especially the left ventricle and right auricle, were greatly dilated, and these two contained ante-mortem clots.

I am sure the experience of others will bear me out as to the frequent occurrence of such sudden seizures of severe cardiac symptoms in the course of chronic valvular disease of the heart, and their occasional occurrence where no signs of valvular disease can be found. You will practically be right in setting these down to acute dilatation. Of course there are causes for such sudden attacks which may appear other than acute dilatation, such as rupture of mitral valve-chords, or retroversion of aortic valves. These conditions, however, produce their evil effects by causing dilatation, so that they rather stand as occasional causes of such dilatation than as alternatives to it.

When regurgitation is suddenly established by any accident to the valves, dilatation of the particular cavity of the heart

which receives the unnatural current is produced by the additional distending cause thus arising. But I have found only a few of the cases of heart disease with sudden and fatal exacerbation to be thus explicable, and have been obliged to conclude that in the majority of cases the cause of the symptoms is some dynamical failure of the heart's muscle—probably a failure to contract at the right moment. In illustration of what may occur here, let us recall a not infrequent cause of a similar difficulty in the case of the urinary bladder. Thus, if a person allows the urine to accumulate in the bladder beyond a certain amount, he may become unable to relieve himself without surgical help, and the over-distended bladder becomes practically paralysed. Now, in like manner, but immensely more suddenly, if you suppose the heart to miss a beat while the stream of blood continues running on into its cavities, these, so far as they are distensible, would at once be subjected to a danger strictly like that in which the bladder often suffers paralysis. In applying this remark, some intermediate considerations must be attended to, for if a single intermission would produce all this effect, sudden death would not be the exception, but the rule. But we are saved generally by the fact that the onward rush of venous blood is reduced to a certain extent by the loss of the beat, although it still continues to run on. So far as the current is slow, it is safe to have a pulse that is slow; but I doubt if we could endure to have an intermission of the time of a beat with the heart at 60 in the course of a pulse at 120. The blood would run on, and in that time would so distend the heart that it never would contract again. So you see that intermissions are of the time of a single beat in the current rate, whatever that is, allowing just that moment's blood to run in excess into the auricle, and no more than one beat is lost. If you lost the time of a beat at 40 in a pulse of 120, there would be at once death by distension-paralysis. Lest you should be unduly alarmed to find your existence hanging by such a thread, however, a safeguard against the occurrence of this over-distension is found in the fact that the sphincter closure of the way out of the heart through the aortic valves is not so efficient as the closure of the neck of the bladder; so that over-distension would sooner force the valves to give way and allow some blood to go on into the aorta. But if at such a time the individual with a weak heart is making an exertion so that the muscular pressure on his arteries is so great as to prevent escape of the blood onwards, then the intermission of the heart may be immediately fatal if the heart is much diseased. So beware of too severe football, for whilst a healthy heart may be able to bear the strain put upon it by the missing of one beat, no doubt

a heart already under difficulties would by such over-distension be paralysed more or less beyond recovery. Is this not the explanation of the very sudden death in some aortic regurgitation cases? It is now becoming generally recognised that, in aortic regurgitation, sudden death is frequent. When aortic regurgitation has distended the ventricle to a certain pitch, an otherwise temporary stay of a beat will become a final cessation of the beating. Any cause which induces more blood to enter the heart in the time of diastole—such a cause as great muscular exertion—would add to the effect of the missing of a beat, and in this way also exertion tends to produce sudden death in these cases.

Should a similar over-distension of the left auricle occur in mitral regurgitation or obstruction, and that cavity thus become greatly widened, the result would not be so quickly fatal, for the auricle is not as essential as the ventricle to some degree of onflow of the blood, enough for life, if too little for comfort; we should thus get distress without immediate death.

It is in dilatation incapable of systole that digitalis exerts its really invaluable efficacy. I venture to say that whenever the heart's cavities are thus dilated, digitalis is the remedy indicated; and this is true whichever may be the valve whose disease may have caused the dilatation. Some writers assume that in aortic disease digitalis is injurious; but, in fact, when dilatation that cannot be closed is reached, *and produces its proper symptoms*, in aortic disease, digitalis is the proper remedy there also. I have found that where digitalis has exerted its benefit to the full, and no more good can be got from it whilst the heart is still giving way, a further check to the symptoms can be obtained by giving tincture of belladonna along with it, in the proportion of ten minims of tincture of belladonna to fifteen of tincture of digitalis. In cases of urgent danger from dilatation with mitral disease, I have given as much as one drachm of tincture of digitalis, in one dose, with marked relief; at other times half a drachm, repeated in two hours.

The direct cause of dilatation of the heart is, of course, necessarily the inward distending pressure of the blood; and where the valvular disease does not encourage this inward pressure, as in simply obstructive disease, then this terrible occurrence of dilatation may be late in arriving. Thus, for the sake of prognosis, it is very important to be able to measure the extent of regurgitation in valvular disease. The pulse does this best. For instance, a full splashing pulse in aortic disease, or a small weak one in mitral disease, is of bad omen, especially the former. But when, in aortic disease, the pulse is small or of moderate volume, the case may do well, in spite of to-and-fro murmurs. Thus, I saw an old gentleman three years ago

whom Dr. Golding Bird had, thirty years previously, warned against running, "because the valves of the great artery of the heart were diseased." He had loud to-and-fro murmurs, but a small quiet pulse, and he died at last of a carcinomatous disorder of the spine. Several such cases I could relate. One man came to me as a life office case, having had a single attack of rheumatic fever fifteen years ago, and no rheumatism at all since that time; he had a loud to-and-fro murmur of aortic disease, but a small steady pulse, and was in good health. Also in mitral regurgitation, as Dr. Barlow used always to insist, the smallness of the pulse as a measure of the regurgitation is a far more important sign than any the stethoscope can give.

Another valuable prognostic sign in heart disease is the *degree of wasting* present, or rather the rate of wasting. While the patient's nourishment is efficient one may hope for him. The texture-life is very sensitive to variations of blood-pressure in some people, and these are they who go to the wall when the heart is in trouble.

One cannot watch a collection of heart cases without being impressed with the evident way in which it reveals the inherent differences of individuals. The imperfect circulation puts a general trial on all the textures, and then we see which bear it well and which ill, and thus come out to view the peculiar tendencies to easy partial decay, which all individuals have more or less—tendencies like the bias in the bowls which Scotchmen roll on grassy lawns to see how near a peg they can go, which bias differs in different bowls, but curves more strongly the course of the bowl as it nears the end of its career. In the same way we find one heart case cyanotic, another jaundiced, another anæmic, another hysteric, according as lung, liver, blood, or nervous system be the weak parts; and as the heart trouble presses more and more these troubles become more severe. It is true that in the long run, as death by stasis of the blood is approached, the conditions becomes much alike in all. But in the prior stages, when minor degrees of dyshæmia are present, I think it is worthy of some remark how long a weaker organ or system in the patient's frame will suffer alone before other and stronger organs give way. A medical friend of mine had aortic disease, with dilatation. He was under my observation for seven years before his death, and during the whole of that time he never was without crepitation at the bases of both lungs, but he never, to the last, had any œdema, nor ever any jaundice. A man now in Guy's has aortic regurgitation of long standing; he has had œdema of the legs for four years, but the bases of the lungs are quite free from crepitation; he has no jaundice. A girl at the present time in Guy's has been eight

times in the hospital during the last five years, and has on each occasion been strongly tinged with jaundice, and has suffered dreadfully with the turbulence and irregularity of an enormously dilated heart, by which for long periods she has been confined to her bed. But in all this time she has never had any crepitation in the lungs, nor ever had any œdema of the feet or legs.

Since being much impressed with these differences, I have endeavoured to learn whether remedies which act in favour of the exceptionally weak and suffering organ have any special power of relieving in these cases; whether, for instance, bile purgatives relieve the jaundice, or pulmonary stimulants the crepitant lungs, or whether it is better to devote all attention to favouring the circulation. And I have been led to the conclusion that much may be done by special care of the weak system or organ towards making proper use of the all-important time in heart disease before dyshæmia has reached the more severe degrees; but that the main objects to keep in view in all cases are these: to maintain the nutrition of the blood (perfectly good blood will almost go round of itself), taking care to keep the constituents of blood plentifully supplied in the diet; to avoid undue strain on the heart itself, inducing the patient to live on one floor if possible; and to keep the texture life as healthy as possible. — *Lancet, Jan.* 12 *and* 19, 1878, *pp.* 42, 80.

19.—SOME CONDITIONS WHICH SIMULATE ORGANIC DISEASE OF THE HEART.

By Dr. J. Milner Fothergill, London.

For every thinking and conscientious practitioner, the question of the presence or absence of structural change in the heart walls is one of the utmost gravity. Such being the case, it may be well to review some conditions which are very liable to be mistaken for organic disease of the heart, and especially fatty degeneration.

Before doing so a few words on fatty degeneration may not be out of place. In the first place, fatty degeneration must be discriminated from fatty accumulation in and upon the heart. Fatty degeneration means the molecular decay of the muscular fibrillæ, which, in advanced cases, are converted into a string of beads of fat. Some fibrillæ are much more affected than others, of course; but, in time, the degeneration spreads so extensively that the heart fails to contract upon its contents, and instant death is the result. Such fatty degeneration is most commonly found as the finale of a long history of hypertrophy with atheromatous arteries, and especially disease of the

coronary vessels. But it is not always so associated; and there may be no evidence of any pre-existing hypertrophic growth. In the absence of such history the diagnosis may be very difficult. The diagnostic indications of fatty degeneration of the heart are put as follows by Dr. Hayden, whose carefulness of observation no one will call in question:—"There is, in all cases, evidence of partial failure of the circulation, under the form of weak, irregular, intermittent, or very slow action of the heart and radial pulse; precordial oppression or pain of an intermittent character, the latter frequently extending down the left arm as far as the elbow; palpitation on making any unusual effort, physical or mental; inability to resist the operation of heat or cold, or preserve the thermal equilibrium under extremes of temperature; pallor of surface; readiness to perspire; and recurrent syncope." The physical signs are the alteration of the first sound of the heart, which is always short and faint: the restriction of these sounds to a limited area within which alone can they be heard; and "the absence of impulse or its extremely feeble character." Such, then, are the subjective and objective phenomena exhibited when the heart is undergoing structural decay.

In this age of physical examination, when a diagnosis rests mainly, and often exclusively, upon physical signs, the subjective phenomena are regarded as of secondary importance; if, indeed, they be not rarely put out of court as witnesses whose testimony is of no importance. With the physical signs as his main guide, the practitioner, when brought face to face with certain conditions, is often in a very difficult position when called upon to answer the question, "Are the heart walls sound?" To this momentous question it is confessedly difficult, in many cases, to make any answer with confidence; and yet it is of stupendous importance to the patient to have an answer, and, still more, a correct one.

Such being the case, it may be well to review those conditions which simulate fatty degeneration of the heart, and which are liable to be taken, or mistaken for it. In the first place, there is a condition of temporary debility of the heart, which is the most common source of error. Stokes goes so far as to say, "It would be difficult or impossible to draw a line of distinction between the signs of simply weakened heart, and this condition combined with fatty degeneration." And this difficulty is experienced by all; and if so great a diagnostician as Dr. Stokes has felt the difficulty, it is no wonder if an ordinary mind occasionally falls into error. Certain it is that there are times when one feels that, so far as the physical signs go, it is impossible to get rid of the fear that there is fatty degeneration in the case. The lack of impulse, and the faint character of the first sound,

are very suggestive; and when to these are added breathlessness upon exertion, inability to sleep with the head low, and a readily compressible pulse, the case looks dark indeed. There is certainly present cardiac debility; there may be also present structural decay. How is the presence or absence of the latter to be made out? Certainly not by the physical signs. We are left then to the surroundings of the case, and these will not always release us from our difficulties. When these signs are found in young anæmic women there is no great difficulty. Walsh says, "This state of things, which I have principally seen in young females, and often in connexion with disordered menstruation, is curable by attention to the states of the uterus, and by the tonic invigorating plan of treatment." It is clear that in conditions of general mal-nutrition of the muscular system the heart must be involved; and its imperfect nutrition will manifest itself by evidences of debility and functional impairment. In such cases, the well-known absence of structural disease, as demonstrated by experience in the dead-house, will suffice to settle the question without much doubt. We know, too, also from pathological experience, that the heart is much weakened in fevers, especially relapsing and puerperal fevers. Indeed, a condition of acute fatty degeneration is produced by a very high temperature maintained for any length of time. Patients recovering from relapsing fever sometimes die suddenly when walking about the wards of fever hospitals, from sudden failure of their weakened hearts. But in these cases there is nothing special, nothing different from the other cases which recover; unless it be in the amount of softening which has gone on in the heart. The heart structures, then, are liable to become degenerate in acute disease, and from this condition they recover as the general convalescence progresses. This form of temporary debility of the heart, with its accompanying signs and symptoms, also is free from any great difficulty in diagnosis. But there are forms of cardiac asthenia where it is next to impossible to say whether there is irremediable degeneration, present or not. They occur in elderly persons whose arteries are far from being above suspicion; where the *tout ensemble* would almost favour the view of fatty degeneration being present. In my experience the case is most frequently of this kind. An elderly person is not very well and calls in medical aid. The complaint is of general languor, and malaise, loss of appetite, disturbed sleep, some thirst, dyspnœa experienced on any exertion, and a certain shortness of breath. There is no pyrexia, and no obvious disease. The pulse is found to be feeble and readily compressed, and it is affected by assuming the erect posture. Naturally, the heart is examined. Its impulse is felt to be absent entirely, or if present, very

feeble. The first sound is very slight and distant, and less audible than the aortic second sound; it also can only be heard over a very limited area. These signs, taken along with the subjective symptoms, will make any man hesitate and doubt. It is prudent, certainly, to be cautious in such a case, and give a guarded answer. Only time and the effects of treatment can demonstrate that the suspicion of fatty degeneration is not well founded, and that the case is being misinterpreted. One very striking case in point occurred to me some time ago in an old gentleman whom I knew well, For my life could I be sure whether his case was one of cardiac asthenia only, or that fatty degeneration was present. However, the general condition was treated by rest in bed, suitable food, and the administration of tonics. In time the heart's sounds became normal; and not only that, but a distinct aortic systolic murmur became audible, and remained so long as I attended him, and was found by the medical gentleman who attended him when I left the neighbourhood. When the old gentleman recovered his usual health, it was found that there was a fairly good hypertrophied heart; which for several years longer struggled so successfully against the aortic stenosis as to permit him to walk about his garden and enjoy life.

At other times, conditions of cardiac asthenia, presenting the physical signs of fatty degeneration, may arise from sedentary labour of a prolonged character, combined with too short hours of sleep. Some four years ago a well-marked instance of this came under my notice. A gentleman, engaged in a business which did not call for much physical exertion, but which entailed long hours of desk work carried far into the night, found himself growing scant of breath, and only able to walk slowly, especially after a meal. He was a stalwart man, weighing some fifteen stones, grey-haired, and gouty. His heart sounds were very faint and feeble, and the impulse not to be felt through his massive chest walls. His pulse was fairly good, being exaggerated by atheromatous vessels. A suspicion of fatty degeneration was not ill-founded in such a case, and indeed a diagnosis to that effect had been made by a physician whom he had consulted. Yet when I saw him I felt great doubts as to whether any fatty decay was really present, in addition to a condition of asthenia. On going carefully into his habits, and finding that he only had a few hours' sleep each night, he was advised to cut down his personal work, and have longer hours of sleep with more exercise. In a short time he lost his disagreeable symptoms, and is now as well as a man of his years, about fifty-five, can well be. It might be maintained that in this case some fatty change was actually present and removed. But that raises the whole question as to whether a

certain amount of molecular decay, where a number of the muscular fibrillæ of the heart walls are converted into fat, the result of imperfect oxidation, may not be recovered from. That by increasing the oxidation the fatty *debris* is removed, and then new material of a healthy character laid down in its stead. This may be so; but we do not yet possess evidence sufficient on the subject to be positive. We do already know that conditions of degeneration in muscles after fracture of one of the long bones are recovered from; and also that the degeneration caused by a prolonged pyrexia soon passes away. It must always remain a vexed question whether, or not, a certain amount of tissue degeneration may not be compatible with repair.

Another form of senile change is also the source of diagnostic error, viz., that of ossified costal cartilages, with emphysematous lung behind them. Here, too, the heart's impulse is imperceptible, and the sounds are distant and feeble, while to percussion little or no heart dulness can be made out. A condition of fatty degeneration is thus simulated, but not very closely. Nevertheless, it has led to mistakes, and a striking illustration occurred to my friend, Dr. Clifford Allbutt, of Leeds. One day the wife of a gentleman in a neighbouring town came to him hurriedly, and desired him to go with her immediately to see her husband. He had suffered from asthmatic dyspnœa for some years, and in the morning, as he did not feel well, and was intending to go up to London that day, he thought he had better call in his family medical attendant. This gentleman made a physical examination of his chest, and pronounced him to have a fatty heart. Another opinion was also called in, and the original diagnosis corroborated. The journey was forbidden, and strict quietude advised. The patient's wife was very much perturbed at this, and hoping that the diagnosis might be erroneous, set off for Dr. Allbutt. When he saw the patient he admitted the physical signs, but pointed out that the pulse was firm and incompressible, and that in all probability there was a large and structurally sound heart behind the emphysematous lung and the rigid costal cartilages. The local medical men handsomely apologised to the patient for their mistake, and the journey to London was proceeded with without mishap. Such a mistake could only take place with practitioners who had been taught, or had taught themselves, to rely exclusively on physical signs, which, however useful ordinarily, on this occasion betrayed them into error.

A much more common condition than any yet described, and often of a still more puzzling nature, is that presented by women at the change of life. Here the condition closely simulates fatty degeneration, and in many cases, in all probability,

there is a blended condition in reality. But much more frequently the condition is one of passing asthenia uncomplicated by structural decay. Such patients are most common in the upper classes. A typical case would present the following features. A lady of indolent habits, not averse from the pleasures of the table, with a taste for tea, and inclined towards less innocent stimulants, but not to any reprehensible length, stout and not rarely pallid, with white and slightly œdematous-looking hands, complains of shortness of breath on any attempt at exertion. She also complains of waking in the night with dyspnœa, and of inability to sleep with her head low, and the necessity for an extra pillow or two. On examination, the pulse is found feeble and easily compressible; the heart's impulse cannot be felt, and its sounds are thin, weak, and distant, while its rhythm is disturbed somewhat. It is impossible by percussion to determine its exact size, but there is every reason to believe that it is somewhat dilated; and it is undoubtedly feeble. The patient complains that she is liable to palpitation on slight effort, or any mental excitement. Palpitation is common, too, at night, and is always present when she is awakened by dyspnœa. But there is another phenomenon which is felt to be much more distressing, and that is a feeling as if the heart was stopping, or indeed had stopped, which is most alarming. Such patients declare that when they feel their hearts palpitating, they are comparatively comfortable, for they know their heart is beating, but that the feeling of stoppage causes acute alarm. In such cases the appetite is defective, and the digestive power low. The bowels are confined, and there is a certain amount of flatulence present. Accumulations of gas in the stomach or in the transverse colon, pressing on the diaphragm and so displacing the heart, cause that organ to beat with difficulty, and so evoke palpitation and dyspnœa. These patients, too, are liable to syncopal attacks, such as are found in actual fatty degeneration. They readily perspire; they have cold extremities, and indeed present the whole array of symptoms which Dr. Hayden gives as those of fatty degeneration. In these cases, from inability to take exercise, from their occupying warmly-heated rooms in consequence of their defective production of body heat, there is undoubtedly defective oxidation of the tissues. From their impaired digestion and loss of appetite there is defective nutrition. The heart, an organ ever in action, is certainly badly nourished—worse nourished, perhaps, than any other part. From the imperfect oxidation, it is very probable that there may be an accumulation of fatty *debris* in its structure as well. It is impossible to be absolutely certain that such is not the fact in any case before one: in many it is

more than probable, in some it is certainly the case. Where there is fatty degeneration blended with the weakened condition of imperfect nutrition, the case proceeds steadily downwards ; the symptoms deepen in gravity, and the end is death.

But in the large majority of cases a less disastrous termination is the result fortunately. The ill-health of the menopause passes away in time; the nutrition improves, and with it the state of the heart walls ; and, after a time, the invalid is found to enjoy fairly good health, and to be capable of a considerable amount of exertion, and little is heard of the once troublesome heart—indeed, the patient is no longer an invalid. Having passed through a period of ill health and some danger, the organism passes into the placid condition of post-catamenial existence, little disturbed by the rhythmic periods which constitute the refrain, as it were, of the catamenial cycles. The appetite returns, and the heart is well nourished. It is free, too, from the reflex disturbances to which it was subject during the menopause, and which often occasioned the fits of palpitation and the syncopal attacks. The recurrent attacks of pallor or of flushing have gradually ceased ; and, indeed, the vascular system is once more free from the perturbations to which it was for a time subject, and which were so alarming and suggestive of serious organic disease.

There is no doubt about the fact of the reduction of a simply dilated heart to its normal size, or near it. And where there has been a condition of debility with dilatation merely, and the heart walls have been structurally sound, such patients recover perfectly. But when there is also some molecular decay the case progresses less satisfactorily. It is probable that a certain amount of fatty degeneration is not incompatible with repair, as in the softened hearts of relapsing fever. How far such decay extends is a matter for the most attentive watchfulness and careful calculation in each individual case. As to what is the exact condition in every case for diagnostic and prognostic purposes can only be ascertained by painstaking examination, and by accurate estimation of each factor in the case. The case of the late Harriet Martineau illustrates what has just been said. Though Sir Thomas Watson and others diagnosed the case correctly, it is clear that a mistake was made by some others, and that the lady herself was under the firm impression that she was the subject of organic disease of the heart. Yet she lived more than twenty years to refute such a diagnosis. The fact that the first sound was described as "noisy," should in itself have put those who examined her on their guard as to the improbability of fatty degeneration in the case. She was at a certain time of life. She had an enlarged ovary as a source of irritation, and her heart was disturbed reflexly. When

the ovary escaped out of the pelvis into the abdomen the perturbations reflexly excited ceased, and the mesmerism got the credit of it. That Harriet Martineau died ultimately of heart disease is very probable. But the reflex disturbances when she was fifty-three years of age surely bore no relation to the disease which existed when she was seventy-four.

As to the murmurs, hæmic or dynamic, which simulate the murmurs of valvular disease little need be said. The hæmic murmur of anæmia is well known; it is situated in the pulmonary orifice, and that is in itself very nearly sufficient to establish its non-organic nature. So also the pulmonary murmur heard when the lung does not sufficiently cover the heart, and aggravated by expiration and lessened by inspiration, speaks for itself. The dynamic murmurs heard at the mitral orifice, which are systolic in time, and which have been so carefully described by Dr. George Balfour, are certainly difficult to distinguish from those of mitral regurgitation, and cannot so be distinguished by the signs revealed to auscultation or percussion. The general features of the case are the only guide as to a correct diagnosis; and it is just the general features of each case, rather than the physical signs, which must determine whether there is present actual organic disease of the heart, or merely a condition which closely simulates it.—*Edinburgh Medical Journal, Feb.* 7, 1878, *p.* 674.

20.—NEUROSAL ANFECTIONS OF THE HEART.

By Dr. J. MILNER FOTHERGILL, Assistant-Physician to the West London Hospital, &c.

There are cases of an aggravated nature, where the whole nervous system is unstrung, but where the chief complaint is of the heart. Such cases are most marked and most commonly seen in women.

In connexion with irritable heart must not be forgotten the effects of tea-drinking. Amongst the out-patients of hospitals this is more especially the case than in private practice; though it is far from unknown in the latter. The active principle of tea—theine—is a powerful neurotic agent, and when indulged in to excess has a very decided action upon the cardiac ganglia, rendering the heart irritable, excited, and unrhythmical in its contractions. In such cases the withdrawal of the tea is absolutely essential to successful treatment. Looked at from a chemical point of view, the principles of coffee and of cocoa are closely allied to those of tea; and it seems difficult to understand how the symptoms produced by excessive indulgence in tea are relieved by substituting for it these other allied vegetable principles. Still clinically the fact remains. It is said that tea

contains, in addition to its principle, theine, a volatile intoxicating oil; and it may be the presence of this agent which makes the difference.

Another vegetable principle exercises a decided effect upon the heart—viz., tobacco. The effect of tobacco is to render the heart's action quicker, its beat feebler, and to promote a liability to palpitation. In the Royal Infirmary of Edinburgh this form of neurosal affection of the heart is recognised and known as "smoker's heart." In many cases this condition arises from great indulgence in strong tobacco; and very frequently the substitution of a lighter form of tobacco in moderation is sufficient to afford relief, without the abandonment of the favourite habit. This form of nervous affection of the heart is not so common, however, as that produced by tea.

In other cases there exists great irritability of the heart, which is closely connected with some irritation elsewhere. Several writers, and especially Botkin, of St. Petersburg, assert the pernicions effect of co-existent irritation elsewhere upon the heart; and insist that such source of irritation be totally removed, or, where that is not practicable, relieved as far as may be. Some little time ago a well-marked instance of such source of irritation came under my notice. The gentleman was the most excitable person imaginable. His heart was going at a pace which defied any correct estimate, but as a rough statement it may be said at about 150 beats per minute, with exalted action. In this case there existed great prostatic irritation, and whenever this was very troublesome the heart's action was always worse. The patient had been at Carlsbad, Homburg, Tarasp, and elsewhere, and been under many leading practitioners at home and abroad without any benefit. He could not be induced to give a systematic line of treatment a fair trial; consequently whether it would have been successful or not cannot be positively affirmed. In such cases there would seem to be either some abnormal activity about the accelerating ganglia of the heart, or else some derangement in the inhibitory action of the pneumogastric.

A case of totally opposite character is supplied by a young gentleman now under my care. He is a tall well-developed youth of active habits, and probably the first derangement of his heart's action took its origin in some overstrain, which has left the heart with perturbed action. He suffers from palpitation, but when seen his heart has always been beating steadily. There is, however, an occasional halt, which is distinctly felt by him, and which he describes as very unpleasant. By accident it was discovered that when set thinking the rate of his pulse became altered. I sent him on to my friend Dr. Broadbent, who found that when talking to him about his case several inter-

missions took place; and on asking a question involving some thought a gradual slowing of the heart's action was produced, but no intermission. He was put upon a combination of bromide of potassium and digitalis, and has improved in every respect. He complains, however, of still feeling intermissions when thinking hard. Here it is obvious that when thinking the vagus is excited to action, and so inhibits or retards the action of the ganglia of Remak.

A distinct class of cases have been described by Da Costa, of Philadelphia, which he has denominated "irritable heart," and which are now well known. They were first noted in men serving in the severe campaigns of the American civil war. The sufferers were unable to march with their comrades, had dizziness and palpitation, with pain in the chest. The pulse-rate was much affected by position, varying from 110 when up, to 80 when lying down. Such cases do not present themselves to our notice in this country in any frequency; and I can only recall one case where the symptoms of irritable heart, as given by Da Costa, were well pronounced. In this case much good arose from the administration of digitalis and bromide of potassium together.

At other times instances present themselves where there is much excitement in the heart's action combined with a generally anæmic condition. Here there is a hæmic murmur at the pulmonary orifice with palpitation at intervals. These cases are usually furnished by girls. Here digitalis and the bromide of potassium are inferior in utility to tonics, with hæmatics, and attention to the leucorrhœa which is almost invariably present. The diagnosis of these cases is not difficult: there is the objective sign of palpitation, the hæmic murmur, the bruit de diable, and the obvious anæmia. The subjective symptoms are—shortness of breath, especially when going up stairs, due to the deficiency of red blood-corpuscles; palpitation; and often general nervousness with vertical headache. In such cases there is often menorrhagia as well as leucorrhœa, and attention to these matters is absolutely essential to successful treatment. Some time ago a young lady was sent to me from Torquay with the above symptoms. Her medical attendant saw that she was pale and anæmic, and found a systolic bruit; whereupon he sent her up to town for further advice. The heart symptoms here were quite a secondary matter, and the true line of treatment to be adopted was that of attention to the general health.

Such are some of the forms of disturbance of the action of the heart met with in practice, without actual organic change being present. They may have different and varied associations; but the source of the disturbed action must doubtless be

sought in the nervous arrangements of the heart itself. These are very much more complex than is ordinarily supposed. There are, first, the cardiac ganglia themselves, by which rhythmical movements can be carried on when the heart is removed from the body. Then, there is, next, the controlling or inhibitory action exercised by the pneumogastric. It is well known that excitation of the pneumogastric nerve will slow the ventricular contractions, and, if powerful enough and sustained, arrest the contractions altogether. In animals it is found that the right vagus possesses this inhibitory action more powerfully than the left. Not only is this the case, but there are contained in the pneumogastric certain fibres which possess an accelerating action, and increase the rapidity of the heart's beat. Irritation of the medulla oblongata will cause an acceleration of the heart's beat if certain nerve tracts are uninjured.

Now, amidst these complex nervous arrangements, it is not always easy to distinguish what part of the mechanism is deranged, and concerned in the production of the symptoms complained of. In cases of suspended systole, or intermittence of theheart's stroke, probably there is some irritation in the vagus by which its arresting action is increased. On the contrary, in these common cases of excited and rapid action of the heart it is possible to speculate at will as to how far this increased action is due to impaired power in the vagus—the regulating and inhibitory power being from some cause diminished—or whether it finds its origin in some irritation which stimulates the accelerating fibres. We are not yet in a positon to speak very dogmatically about such nervous derangements—with their objective symptoms, palpitation, and unrhythmical action. Probably in a few years more this department of heart ailments will become as distinctly intelligible as are the other forms of diseases of this organ; at present each case forms a study of itself and requires its own appropriate treatment. Consequently it is not possible to lay down axiomatic rules for treatment, as has been done in the preceding articles on primary and secondary affections of the heart. All that may be said is that in many cases relief, and even something more, can be attained by a scheme of medication which consists in the administration of tonics, together with sedative neurotics, as in the union of quinine and digitalis with the bromide of potassium or hydrobromic acid. In addition to these measures it is obviously necessary to place the system as far as possible at rest, both physically and psychically; and this is to be achieved largely by the avoidance of all forms of disturbance and excitement, especially of a sexual character. The demands upon the system must be reduced to a minimum. We all recognise how

important it is to reduce all demand, such as is induced by exertion or effort, upon the heart when its muscular walls are affected. So in neurosal affections of this organ, it is equally desirable that all forms of nervous expenditure be economised to the utmost. In all cases where there are obvious derangements these must be attended to. A misplaced uterus must be replaced; the leucorrhœal flow, so commonly found, must be attended to; if there be any tendency to a load in the bowels this must be done away with by a proper purgative; if there be any dyspepsia present it must be met by a suitable diet, and vegetable bitters with bismuth; whenever there is anæmia some of the lighter preparations of iron must be prescribed, in small doses at first, and always after food. Mental and physical quiet must be insisted upon; and still more, for successful treatment, the patient must be made clearly to understand that the physician is confident in his power to afford relief.—*Lancet, Dec.* 15, 1878, *p.* 878.

21.—CASE OF PROLONGED SYNCOPE WITH CEREBRAL DISTURBANCE, TREATED BY NITRITE OF AMYL.

By Dr. WILLIAM O'NEILL, Physician to the Lincoln Lunatic Hospital.

On the 4th of September I was sent for in great haste to see Mr. W., who, it was represented, was in a fit. This gentleman, who is about 65 years of age, has been under my care, on several occasions, for disease of the heart (hypertrophy and dilatation) with great irregularity of the pulse. On the day above stated, being much fatigued from walking and transacting business, he called at the house of a friend, to one of the inmates of which he complained of pain and a sinking sensation in the region of the stomach, and whilst some stimulant cordial was being prepared for him, he sank on a seat, leaned forward, attempted to vomit, and then became insensible. When I saw him about half an hour after the seizure he was sitting supported on a couch by a person who sat beside him, and who prevented his head from falling forward on his chest. He was quite insensible, and breathed in a peculiar shallow way, giving a deep gasp or sigh every few minutes. The eyes were shut, and the countenance had assumed a deathlike pallor. The head, extremities, and indeed the whole body felt as cold as ice, and were saturated with clammy sweat. The pulse could not be felt, but I could hear the heart beat faintly and irregularly. The teeth were firmly clenched, and he had passed a most offensive motion under him.

For several hours the patient was not moved from the sitting

position in which I found him for fear of extinguishing the little breathing power left. From this cause then, and from his insensibility, and from the clenching of his teeth, I was not able to give anything—either by the mouth or anus—but the extreme coldness of the patient suggested the application of warmth to the surface, and very fortunately the appliances for doing so were at hand. I therefore ordered his feet and legs to be gently put into a deep tub of hot water and mustard, his hands and arms to be laid on a hot oven shelf enveloped in flannel, his thighs and body to be surrounded as far as practicable with bottles of hot water, and ammonia to be held to the nostrils. Whilst all this was being done I procured some nitrite of amyl, but as I was about administering it the patient's breathing ceased, and I, as well as the relatives about him, thought he was gone. After some seconds, however, he gave a deep gasp, and that gasp drew a good whiff of the amyl into his lungs, and the breathing, such as it was, to our satisfaction was again restored. At first I administered the amyl on a handkerchief, gradually giving larger and more liberal doses, but finding that I did not make very much progress in this way I applied the unstoppered bottle to the nose. I thought, as the saying is, I would give the medicine "a fair trial," and a fair trial I gave it, to the saving, I believe, of the patient's life. In this way, off and on, from four o'clock in the afternoon till nearly twelve o'clock at night, in order to keep up its good effects, I kept administering the amyl, and I believe I could not have used much less than two drachms of it from first to last. Until I gave it from the bottle freely there was no very marked appearance of flushings of the face, or much amelioration of his state, but with the flushings came gradual improvement of symptoms, and gradual heat of head, hands, feet, and body. About ten o'clock the breathing and general condition had so much improved we were able to lay him down on a couch with his head on some pillows, and about the same time he was able to swallow a few teaspoonfuls of fluid, but perfect swallowing and speech had not returned till seven o'clock on the following morning. At nine o'clock, when I called, his breathing and pulse were better than usual. He was feverish and his face was as red as scarlet. The redness of the face and the feverish state remained for a day or two, and then gradually disappeared.

In the treatment of this case the warm applications were most salutary adjuvants, but to the repeated and persistent use of the nitrite of amyl I attributed mainly the gentleman's recovery. It is now nearly three years since I first saw the great benefit which could be derived from the judicious employment of the drug, from having had occasion to administer it

by inhalation to a patient suffering from intense renal dropsy with great orthopnœa, the breathing during the last few weeks of this patient's life assuming that character first described by Dr. Cheyne. This poor man derived the greatest comfort and relief from time to time from the inhalation of four or five drops of the amyl. On several occasions it seemed to restore and reinvigorate in a most extraordinary way the flickering breathing when it was apparently about to cease.

I do not make any comment as to the nature of the seizure in this most interesting case of Mr. W., further than to say that from the beginning of the syncopal attack to the end of it the patient presented no symptoms of paralysis either local or general, and that the cerebral symptoms were caused, I believe, more from a deficiency of blood to the brain than by a superabundance of that fluid.—*Practitioner, Dec.* 1877, *p.* 401.

DISEASES OF THE ORGANS OF RESPIRATION.

22.—TREATMENT OF COLD IN THE HEAD BY SALICIN.

By Dr. T. A. Maclagan, Dundee.

This most troublesome ailment (Coryza, rhinitis) is one which we are seldom called on specially to treat, partly because (except in infants) it is not a source of anxiety, and partly because every one knows that remedies are of little avail in shortening the duration or mitigating the severity of the attack.

The following cases are given in the hope that others may be induced to try the treatment by salicin, and that my experience may not be singular.

Case 1.—Occurred in my own person. In February, 1875, I had a bad cold in the head; felt oppressed, and miserable, and stupid; the forehead felt dull and heavy, the eyes were watery, there was frequent sneezing, and some running at the nose. But why describe the symptoms? every one knows them. And every one knows too that they generally last for several days, if not for a week.

I was making observations on salicin at the time. It seemed to me (for reasons which I need not stay to give here) that salicin was a very suitable remedy for my ailment. I therefore took twenty grains every two hours. I took the first powder at eight in the morning, and continued to take it every two hours during the day. After the third dose I was quite sensible of feeling better; and when evening came, having taken in all 160 grains, my cold was gone—I felt quite well.

I took forty grains at bed time, and got up the next morning

perfectly well. As a precautionary measure, I took the salicin in twenty grain doses four times a day for a couple of days. I did not use any other means to get rid of the cold, and did not even remain in the house.

I have frequently had such colds, but never got rid of one so quickly before.

Case 2.—My little boy, aged six, had a bad coryza, with all the usual symptoms. He rose with it on him on the morning of the 6th May, 1876. I gave him eight grains of salicin every two hours; and by evening when I saw him again, having had six powders, (forty-eight grains) his cold was nearly gone. The next morning he was quite well.

I would simply note that it is not every catarrh, but *cold in the head* that is benefited by salicin.

The drug is a perfectly safe one, and may be given in large dose with impunity. It is so quickly eliminated from the system that its full beneficial action can only be got by giving it in frequently repeated dose. For an adult fifteen to thirty grains every hour is not too much where it is desired to get a speedy action.

It is a very pleasant bitter, and is best taken stirred up with about an ounce of water in a wine glass, a little syrup of orange being added for those who like it.—*Practitioner, Nov.* 1877, *p.* 325.

DISEASES OF THE ORGANS OF DIGESTION.

23.—CASE OF ASCITES AND ITS SUCCESSFUL TREATMENT.

By Dr. John Kent Spender, Bath.

[The patient was a man 54 years of age, who had always enjoyed excellent health. After the existence of minor symptoms for a short time, his belly began to swell, and in quick succession the thighs, the legs, and the feet. When first seen by Dr. Spender,]

The abdomen was greatly distended with fluid, and measured 38¾ inches in circumference at the level of the umbilicus. There had been no action of the bowels for two or three days. A small quantity of dark turbid urine was shown to me. There was no appetite for food, and scarcely any had been taken for some weeks. The thighs, legs, and feet were extremely œdematous. The dyspnœa was severe and increasing, and hardly any sleep was obtained night or day.

But the dark picture had a bright side. Careful auscultation of the heart discovered no murmur, and not even an irregularity of action, The lungs seemed healthy in every part, except that

some basic *râles* could be heard. There was no evidence of any disease of the large blood-vessels. The intellect was clear, nor were there any signs of aberration in sensory or motor functions. Literally it seemed as if the upper half of the body were entirely free from disease, and as if the organs which it contained did their duty in almost physiological perfection.

The immediately pressing indication was to cause a free action of the bowels. Two powders were ordered, containing jalap, gamboge, and elaterium; they were taken at an interval of twelve hours, and caused several large watery evacuations. A mixture containing acetate of potash and chloric ether was taken at the same time. The patient was kept in bed in a warm room, and allowed an abundance of milk and weak tea (nothing else) as his diet.

On Monday, the 28th, there was a slight improvement. The relief of the bowels had lessened the dyspnœa, and more urine had passed. The abdomen measured half an inch less than on Saturday. The mixture and powders were repeated; a powder to be taken every night.

On March 3rd my patient was decidedly better. The effect of the medicines was most beneficial. Fluid food was readily taken, and seemed to be digested. To-day I carefully examined the urine: it contained albumen to the extent of about $\frac{1}{10}$th, and the sp. gr. was 1024. There was no sugar, and the reaction was only slightly acid. Examined under the microscope, the urine presented blood-globules, and what looked like small "waxy casts."

The improvement was sustained on the 9th inst., and I was able to pronounce with certainty that there would be no rapidly fatal issue. The albuminuria was diminishing. I prescribed a combination of tartrate of soda and tartrate of iron: the former in such doses as to secure a continuously laxative action of the bowels. One of the old powders was now given on every third morning.

Six days afterwards, the abdomen measured thirty-seven inches in circumference, and the swelling in the lower limbs was clearly going down. Broth was allowed in addition to the milk. As the powders were nauseous and caused sickness, the following pills were ordered:—Ext. elaterii, gr. ij; p. gambogiæ, gr. vj; p. capsici, gr. vj; ext. gent. q. s. M. ft. pil. vj. primo mane p. r. n.

On the 21st inst., it was evident that the iron mixture did not agree, and the old acetate of potash mixture was substituted for it. Some solid food was permitted.

On March 27th, 15 minims of tincture of digitalis were added to each dose of the diuretic mixture; and on April 8th I added a little decoction of broom. The pills were continued at regu-

lar intervals. During these few weeks the size of the legs and thighs steadily went down, and the health so far improved that E. H. was out of bed for some hours every day; but the ascites was very slowly reduced. Consequently on the 11th of April I drew off by tapping twenty-two pints of thin straw-coloured fluid. He bore the operation well, and was much relieved. Immediately afterwards the abdomen was bandaged with moderate tightness, and an opiate draught was given every six hours. During the next few days the flow of urine increased to four pints in twenty-four hours, and the albuminuria proportionately decreased.

I took the earliest opportunity of making a careful physical examination of the abdomen. The liver was "very slightly enlarged"—these are the words in my notes of the case; but beyond this I was unable to discover anything unnatural. It was in one sense disappointing to find so little sign of morbid change; but it gave a backbone to a favourable prognosis. My patient regained his strength rapidly, and was out of bed for several hours every day. His daily consumption of milk amounted to two quarts, and it seemed to be well digested. I now kept his bowels sufficiently moved by half an ounce of castor oil on alternate mornings, changing it sometimes for three drachms of sulphate of magnesia (dissolved in half a tumblerful of water). Ten grains of iodide of potassium were added to each dose of the acetate of potash and digitalis mixture. On the 19th of April, 30 minims of tincture of perchloride of iron were substituted for the digitalis and iodide of potassium. This medicinal treatment was continued without alteration until the 12th of May; but the abdomen rapidly refilled. And about this time he "took cold," which confined him to bed for a few days, and obliged me to stop the tonic medicines.

On the 28th of May my patient was again tapped, and eleven pints of fluid withdrawn. As soon as possible the iron and acetate of potash mixture was resumed, and the bowels kept slightly relaxed by the same plan as before. After the 10th of June the iron was given without the alkali, and to retard the re-accumulation of fluid in the abdomen an occasional brisk purgative of podophyllin and elaterium was taken at night. As the weather had now become very warm, E. H. was allowed to have a walk out of doors every day, and he rapidly got back flesh and strength.

And so things went on prosperously through the summer with this noteworthy point, that the fluid returned in the abdomen to a small extent (under three pints, as nearly as I could guess), and then stopped. The œdema had entirely left the lower limbs, and he could walk as well as ever. The memor-

able heat of July and August suited him exactly. I saw him about once a fortnight, and on my arrival home after a short holiday at the beginning of September he was well enough to go to the sea-side. The ascites had so receded as to be practically non-existent, and there was only a trace of albumen in the urine.

During the past twelvemonth I have seen E. H. at regular intervals, and on the last occasion (September 9, 1877) he seemed in perfect health.

Commentary.—Ascites is a symptom which by its very magnitude may be called a disease. It is an effect which becomes a cause of other effects, scarcely less important and abiding. And so intricately are the static and dynamic phenomena of ascites bound up together that the keenest clinical insight cannot always separate and classify them.

When a medical man is called to a case in which abdominal dropsy is a prominent feature, he may be excused for some hesitation in giving a confident diagnosis. The abdominal viscera are under a watery veil, and perhaps can be hardly felt or even dimly discerned. There are, however, two points the determination of which is of high importance, and will to a large degree govern the prognosis. What is the condition of the heart and of the kidneys? The pathological interdependence of these organs is a medical proverb; but we cannot always see at the first glance which organ was originally at fault. In the present case, the heart was above all suspicion. Beyond mere weakness neither its valves nor its muscular apparatus could be proved to have a flaw. There was nothing demonstrably wrong about the other thoracic organs. The upper limbs were free from dropsy. There was no reason to imagine albuminuric retinitis, and of subconjunctival œdema there was not a trace. And there never had been any approach to delirium, or the slightest impairment of the intellectual powers.

Coming then to the part of the body below the diaphragm, the fact that the dropsy began in the abdomen would of itself free the heart and lungs from any complicity in the matter. On interrogating the kidneys, there seemed little reason to believe that those organs had suffered any serious morbid change, The scanty secretion of turbid urine was probably due to the backward pressure of the ascitic fluid upon the renal blood-vessels: and it was fair to hope that the albuminuria was only of the congestive kind. The ulterior history of the patient proved that this view was strictly correct; for the function of the kidneys was restored in exact proportion to the removal of the dropsical pressure from them. The œdema of the lower limbs was capable of a similar explanation, and was obviously due to the tremendous pressure upon the great veins within the

abdomen. The state of the liver seemed, at the outset of the inquiry, a cardinal point for investigation. But although the long tropical or sub-tropical residence of the patient suggested the theory of structural mischief in the liver, yet during the fourteen years that he had subsequently lived in this country there had been no illness which could be fairly attributed to this cause. His uniform sobriety and steadiness of conduct, winning for him terms of eulogy such as one seldom reads in the "discharge-papers" of any soldier, negatived the possibility of cirrhosis from alcoholic drinks. There was no material enlargement of the superficial veins of the abdomen, and certainly no approach to jaundice. There was little substantial evidence of any other disease of the liver, except perhaps a casual perihepatitis; and after the abdomen had been emptied by tapping, I could detect no enlargement of the spleen.

By a process of exclusion, therefore, I was compelled to think of an altered condition of the peritoneum itself as the probable source of the ascites. The hypothesis of strumous or malignant disease of the peritoneum was dismissed as unlikely, as no tumour or thickening could be detected in the abdomen, and the patient had no signs of a scrofulous or a cancerous diathesis. That the effusion was the result of a slow inflammatory action, seemed to be most in accordance with the clinical facts, and with the way in which the illness began. It is remarkable, however, that during my long attendance there was no indication of any pyrexia.

Dr. Hilton Fagge says that the prognosis of almost all cases of ascites is very unfavourable; but he adds that when recovery occurs, the great probability is that the dropsy was the result of either a chronic peritonitis or a perihepatitis. Most practitioners must be familiar with those cases of the disease in which repeated tapping affords only temporary benefit, and death happens sooner or later from loss of appetite and general exhaustion. The more frequently we relieve the patient of his watery burden, the more quickly does the fluid come back again; and our very anxiety to bring present ease hurries the sick man to the sure and certain issue of a "watery grave." Dr. Murchison alludes to particular cases which seem to constitute a connecting link between inflammation and dropsy of the peritoneum where the attack begins with symptoms of sub-acute inflammation, but subsequently takes on more the characters of dropsy. The effusion in these cases often rapidly disappears under treatment.

A few words will be sufficient commentary on the therapeutic management of this case. Clearly the first indication was to establish a copious flux from the bowels, by which the congestion of many organs might be relieved. As an adjunct

to other drastic purgatives gamboge was long ago recommended by Sir R. Christison; and in the present instance it seemed very useful. The digitalis was prescribed with the view of augmenting the blood-pressure, according to the doctrine lately propounded by Professor Ackermann, of Rostock. A greater blood-pressure means a quicker circulation through excretory glands, and a more continuous process of elimination. And digitalis would help to correct any of the dropsical condition which might happen to depend upon mere feebleness of heart. The long administration of iron was not only of benefit to the general health, but distinctly promoted the absorption of the residual ascites after the second tapping. Lastly, it may be mentioned that I have always followed Dr. Basham's plan of bandaging the abdomen after paracentesis, and it seems to do good both by affording support and by checking the re-accumulation of fluid.—*Practitioner, Jan.* 1878, *p.* 5.

24.—OXIDE OF ZINC IN INFANTILE DIARRHŒA.

By Dr. J. Crawford Renton, Assistant-Surgeon, Eye Infirmary, Glasgow.

When we consider the rapid development which is going on in the organs of the young child, it does not surprise us to find that diarrhœa, in one form or another, is one of the chief ailments to which young lives are liable.

A variety of forms have been tabulated, but when actual cases present themselves it is not always easy, nor is it necessary, to place each under the special division or sub-division which some authors consider essential to an intelligent practice of our profession. For convenience and perspicuity in writing it is, however, best to adopt some classification, such as, the Simple and Non-Inflammatory, which includes the Nervous, and the Inflammatory.

The class of cases to which we wish specially to refer is more directly Nervous, but like those which neither cure themselves nor are cured by medicinal or dietetic interference, it is liable to become inflammatory, thus merging into the second class.

When the nerves governing the gland secretions have been irritated, and have lost their natural power; when a sudden fright or other remote cause has produced a derangement of the nervous system; when the stomach and intestines have had their tonicity augmented, or, in other words, when they contract too soon on the undigested food; we have as a result what has been called by some lienteric diarrhœa, that is to say, that along with copious fluid evacuations pieces of undigested food are passed.

So spasmodic are the evacuations in some cases that we might almost say we had an intestinal chorea.

We come, therefore, to consider what is the most suitable remedy to employ in such cases. While acting as assistant in the Royal Hospital for Sick Children in Edinburgh, considerable opportunities were afforded for testing the value of different drugs.

Among those tried were:—Bismuth and oxalate of cerium, both of which we found useful, the latter especially where vomiting was a prominent symptom; Dover's powder in combination with a little grey powder in some cases acted like a charm, but in the severe forms of the class under consideration was of little use; kino powder, mineral acids, stimulants in the shape of spirit of camphor, brandy, ammonia, &c., were all of service; sedatives, as chloral and bromide of potassium, which produced a soothed condition of the nervous system, thus giving rest, which is one of our most valuable curative agents. We found also that by careful dietetic treatment, consisting principally, in young children, of small quantities of milk and lime water often repeated, considerable improvement took place.

Anxious, however, to obtain some drug upon which we could rely even more than on those mentioned, oxide of zinc had been recommended by Dr. Brakenridge, Physician to the Hospital, and Assistant-Physician to the Royal Infirmary in Edinburgh. Oxide of zinc is described as tonic, sedative and astringent, and when it was considered that it had enjoyed a high reputation in nervous complaints, such as spasmodic asthma, chorea, certain forms of convulsions, and had been employed with success in chronic dysentery, as also in chronic bronchitis, we were hopeful that in infantile nervous diarrhœa, or, as we are more inclined to call it, intestinal chorea, it might prove serviceable, and, accordingly, it received a trial in a large number of cases, the result of which was published at the time by Dr. Brakenridge.

The plan adopted at first was to give to a child of six months two grains of the powder every six hours, and generally after the third dose a distinct improvement was observed. As it was inconvenient in the form of powders, more especially for hospital patients, it was made up as a mixture with mucilage and water. The question, however, immediately arose, Might the good effects not be ascribed in many cases to the mucilage and water? To obviate any fallacy such as this a certain number of cases were treated by water, and also some by mucilage alone; the result being that drachm doses of water every six hours were followed by amendment in the mildest cases, while mucilage produced a good effect where the symptoms were by no means mild. Where, however, the purging was severe we

were compelled to fall back on the oxide of zinc, which checked it at once.

During two weeks it suddenly failed, and we feared that, like some other medicines which seem to do well for a time, it had ceased to be so satisfactory, but on enquiry it was found that sugar instead of mucilage had been used in making the mixture, which at once accounted for the temporary failure.

Cases which had gone the length of the dysenteric type yielded to this treatment, and as case after case was benefited by it we saw that in oxide of zinc we had a most valuable agent for the treatment of this form of infantile diarrhœa. Several cases are fresh before us where the child was *in extremis*, and only death was looked for, and carefully administered doses of the zinc, with proper dietetic treatment, which must of necessity accompany all medicinal medication, were followed by the best result.

It is by no means easy to have a clear conception as to what any medicine does, and by its effects we can only judge. Oxide of zinc is called a tonic to the nervous system, we know that tonics are those drugs which improve the condition of the economy; whether zinc supplies a molecular defect, or acts on the blood supplying the nerves in the neighbourhood of the part affected, or is more central in its influence, we are not in a position definitely to say. According to some its action is purely mechanical. It seems, however, in the instance before us, to act by steadying the nervous system, curbing it as it were, and restoring it to a state of quiescence. While we recommend the above treatment, we hold strongly at the same time the great importance of combining rest and improved diet, along with suitable drugs, if we wish successfully to combat this disease.—*Glasgow Medical Journal, July* 1877, *p*. 341.

DISEASES OF THE URINARY ORGANS.

25.—ON THE ALBUMINOUS SUBSTANCES WHICH OCCUR IN THE URINE IN ALBUMINURIA.

By Dr. T. Lauder Brunton, F.R.S., and Mr. D'Arcy Power.

White of egg, or albumen, has given its name to the substances which more or less resemble it in their chemical reactions, and they are therefore termed albuminous. But white of egg contains other substances besides the constituent which gives its characteristic properties to it and forms the chief part of its bulk. To distinguish between the crude white of egg, or albumen, and its chief constituent, egg-albumin, the former is spelt with an *e*, album*e*n, and the latter with an *i*, album*i*n.

The class of albuminous substances contains bodies which differ so much from each other that it has been found necessary to subdivide it into several groups. Hoppe-Seyler gives eight groups, which are—I. Albumins, II. Globulins, III. Fibrins, IV. Albuminates, V. Acid Albumins or Syntonin, VI. Amyloid, VII. Coagulated Albuminous Bodies, VIII. Peptones.

Two of these groups, viz., fibrins and amyloid substance, are only met with in the solid form, so they do not concern us in an inquiry into the nature of the albuminous substances which occur in solution in the urine. A third group, viz., coagulated albuminous bodies, also occurs only in the solid form. Five groups, therefore, remain which occur in solution, and may consequently appear in the urine. These are albumins, globulins, albuminates, acid-albumins, and peptones. We may further classify these five groups into three divisions. *First*, albuminous bodies in what, for convenience' sake, although not perhaps strictly accurately, we may term their natural condition, in which they are coagulated by boiling. This division contains albumins and globulins.

Second, Albuminous bodies in combination with acids and alkalis, and not coagulated by boiling. Albumins and globulins both combine with mineral acids and alkalis to form acid-albumins and alkali-albumins, or alkali-albuminates, as they are generally called. Thus, if we take a little white of egg dissolved in a quantity of water, we get a solution which is coagulated by boiling. But if we first add to it some very dilute nitric or hydrochloric acid (*e.g.*, its own bulk of four parts commercial acid in 1000 of water), and then heat it, we may boil it as much as we please, but no coagulum will form. The albumin has combined with the acid and formed acid-albumin, which is not coagulated by heat. This is the reason why carelessness in washing out test-tubes sometimes causes the presence of albumin in urine to be overlooked. Let us suppose that a man tests urine in the usual way, either by boiling and adding nitric acid afterwards, or by nitric acid alone, and afterwards throws out the mixture of urine and acid. He then pours some fresh urine into the tube without washing it and proceeds to boil. The urine remains clear, and he supposes it to be free from albumin, yet it may be highly albuminous. For the acid diluted by the urine first tested clings to the side of the tube, and being thus heated with the second urine, gradually converts it into acid-albumin; and by the time the coagulating point of the unchanged albumin is reached, there is no longer any to coagulate, the whole having been changed into acid-albumin. By boiling a solution of white of egg or some albuminous urine with liquor potassæ, instead of dilute acid, the albumin in either solution will be converted into

alkali-albumin, or alkali-albuminate, and will not be coagulated by boiling. By its conversion into acid-albumin or alkali-albumin, ordinary albumin undergoes another change besides the loss of its coagulability on boiling, for it loses also its solubility in water. White of egg, or the dried albumin from serum or urine, may be dissolved in water and give a neutral solution, but after it has been changed into acid-albumin or alkali-albumin it becomes insoluble in water, and is therefore precipitated from its acid or alkaline solutions by neutralising them. When the point of neutralisation is passed, and the solutions rendered alkaline by alkalis or their carbonates, or acid by mineral acids, the precipitate is redissolved, but will again be thrown down by neutralising. But if acetic acid be used instead of a mineral acid to neutralise a solution of alkali-albuminate, the precipitate is not dissolved by a slight excess of acid. Indeed, when sodium phosphate is present in the solution, alkali-albumin is not precipitated by exact neutralisation and the precipitate only falls after the liquid has been rendered acid. When we wish, therefore, to separate alkali-albumin from a liquid, we acidulate with acetic acid. By then boiling we can precipitate both the ordinary albumin and the alkali-albumin from a fluid which contains them, while if we boiled without previously adding acetic acid, the ordinary albumin only would be coagulated, and on removing it by filtration the fluid would be found to contain alkali-albuminate. But besides its use in precipitating alkali-albuminate, acetic acid possesses the power of causing ordinary albumin to coagulate more readily on the application of heat, and its addition to any fluid from which we wish to separate albumin thus serves a double purpose.

The *third* section into which we have divided soluble albuminous bodies contains only one group, that of peptones. These are albuminous bodies so much altered by the process of digestion that they are neither coagulated by heat nor precipitated by neutralisation. They are, however, precipitated by alcohol.

The presence of albumin in the urine is universally acknowledged to be a morbid condition, but its causation and significance are very varied. At one time it is a symptom of the gravest importance, at another it may be of very little consequence. This fact is of itself sufficient to show that under the general term albuminuria many dissimilar conditions are grouped. Some of these have already been dissociated, and we recognise the distinction between the albuminuria of Bright's disease and that dependent on cardiac lesions. But besides these there are probably other forms of albuminuria, less common and less important, but yet deserving

of more attention than they have hitherto received in respect to diagnosis, treatment, and prognosis. It was with the view of attempting to distinguish these, as well as with the hope of gaining some new insight into the ordinary forms of albuminuria, that we began our present research. The graat differences which are observed in the behaviour of albuminous urine when boiled or treated with nitric acid have led medical men to recognise that the albuminous bodies occurring in urine are not always the same; that two or more kinds of albuminous bodies may sometimes be present in the urine at once. Lehmann showed that paraglobulin is generally present in albuminous urine along with serum-albumin, and his results were confirmed and extended by Edlefsen and Senator. These observers dealt chiefly, however, with the albuminous bodies normally present in the blood, although Senator observed the presence of peptones. It is to the classic researches of Stockvis that we owe the first clear demonstration that albuminous bodies may be absorbed from the stomach and intestines, and excreted unchanged in the urine. Numerous experiments showed him that the serum of blood and the albumen usually excreted by the kidneys in Bright's disease were identical, and that when either of them was injected under the skin, or directly into the vessels of the animal, provided that the experiment was conducted in such a manner as not to disturb the circulation, the urine remained quite free from albumin, the healthy kidneys apparently refusing to let the albumin pass through them. But when egg-albumin, or Bence-Jones's albumin (a curious kind of albumin obtained from the urine in cases of osteomalachia) was used, the result was very different, for both of these seemed to pass readily through the kidneys, and appeared again apparently unchanged in the urine. When taken into the stomach, raw eggs, as every one knows, are digested, and egg-albumin does not appear in the urine as a rule; but if the quantity of eggs has been too great for the digestive powers, the albumin is absorbed, and appears in the urine. Both egg-albumin and Bence Jones's albumin are absorbed from the rectum, and appear in the urine. Nor is it only undigested albuminous substances which are thus absorbed and excreted. Claude Bernard observed that after partaking of a quantity of *cooked* eggs, his urine became albuminous. The coagulated albumen of cooked eggs could obviously not be absorbed without undergoing some previous change. But Kühne has found that the pancreatic juice, before converting coagulated albuminous bodies into peptones, seems to change coagulated albuminous bodies into something resembling their raw condition before converting them into peptones. In Bernard's observation the cooked eggs which he swallowed seem to have

undergone this change, and then been absorbed in the same way as raw eggs would have been. The fact that absorption of albuminous substances does take place from the intestine makes it appear extraordinary that albumin is not more frequently found in the urine during digestion, and one can only suppose the reason to be that it is only when the digestive powers are overtaxed, as by swallowing many raw eggs together, or deranged so as to digest the food partially but not completely, that such an event occurs. In a clinical lecture, published in the "Medical Times and Gazette," for April 10, 1852, the late Dr. Parkes noticed that in cases of albuminuria the albumin was much increased after meals, and he ventured the hypothesis that the albumin was of a different quality, as well as increased in quantity. In the same journal, April 22, 1854, he discussed the origin of this increase, and distinguished it by the name of food-albuminuria. This food-albuminuria he considered was not due to congestion of the kidneys during digestion, for the water of the urine is often diminished, and the solids do not increase in proportion to the albumin. He therefore thought it might be due to albumin not being altered in the stomach and liver, and therefore being eliminated like white of egg. He called attention to the fact that the antecedents of Bright's disease are often such as to impair the functions of the stomach and liver, and that dyspeptic symptoms often appear before renal. In his work on the urine he also quotes the case described by Christison of a young man in whom cheese always produced temporary albuminuria, and who afterwards died of Bright's disease.

Similar observations to those of Parkes were made about the same time as his by Gubler, who communicated to the Société de Biologie, August, 6, 1853, his observation that the amount of albumin was increased during digestion. He proceeded to try the effects of various diets on the patients, and found that, with an exclusively vegetable diet, the albumin sank to a minimum; that it reached its maximum on an exclusively albuminous diet, and was intermediate in quantity when the diet was mixed (article in Dictionnaire Encyclopêdie des Sciences Médicales, 1865, tom. ii. p. 447).

This subject was again taken up by Dr. Pavy, who confirmed Parkes' results regarding the increase of albumin in the urine during digestion, and tried to ascertain experimentally whether or not Parkes' supposition were correct that the albumin found in the urine varied in character as well as in quantity at different times. It occurred to him that its occasional presence in the urine might be due to its diffusibility being greater at one time than another. He therefore employed a dialysing apparatus to distinguish between the albumins, with the result of

showing that very considerable differences exist in the readiness with which different specimens of albuminous urine pass through animal membranes. We quote the following passage from his Gulstonian Lectures for 1862 (Lancet, May 23, 1863): "On submitting some specimens of albuminous urine to dialysis, I encountered one where the albumin passed in considerable quantity. The urine was derived from a patient affected with phthisis, who never had experienced any symptom of dropsy. It was highly charged with albumin, and on being submitted to dialysis, using vegetable parchment as a septum, the albumen passed to such an extent that the diffusate, after twenty-four hours (distilled water had been placed on the other side of the membrane), gave a pretty copious precipitate with heat and nitric acid, as also with the yellow prussiate of potash and acetic acid test. It was noticed, as happens with some specimens of albuminous urine, that the precipitate produced in the diffusate, by the addition of a small quantity of nitric acid, was redissolved on agitation. A considerable excess of nitric acid was required to throw down a permanent precipitate. The same result was obtained on several occasions, and no idea could be entertained as to any imperfection in the dialyser, because the same one was used for blood and other specimens of albuminous urine, but albumin in neither case passed in twenty-four hours to any sensible extent. The patient left the hospital, and was readmitted six months later almost in a moribund state. His urine was still highly albuminous, but curiously enough, now gave scarcely any positive result on being submitted to diffusion. Dialysed for forty-eight hours with a septum of vegetable parchment, the diffusate yielded only the slightest turbidity on being tested for albumen. Although I have met," he continues, "with differences in other cases, still with the specimens of urine I have as yet examined I have never encountered one where the albumin diffused to an extent at all comparable to that in the case I have just mentioned." Interesting as these observations of Pavy were, they do not seem to have been taken up and extended, probably because the method of determining the nature of the albumin was too troublesome to allow of its being applied readily in the sick chamber or hospital ward. In our present research we have endeavoured to distinguish the different albumins in urine by determining their coagulating points, because we wished to employ a method which could be used at the bedside with such ease that, if it gave any useful indication at all, it might be universally employed.

The method simply consists in holding a thermometer in the urine while it is being gently heated in a test-tube over a spirit lamp, and noting the temperature at which the urine

begins to grow milky from the commencing coagulation. Instead of holding the thermometer with the fingers, it may be fixed in the test-tube by means of a conical india-rubber stopper, with a hole in the middle through which the thermometer passes. As one is apt to break the thermometer in pushing it into and drawing it out of the hole, it is well to split the stopper to its middle along its whole length. By opening the slit the thermometer can be put in and taken out, or its position altered, with the greatest ease, and the conical form of the stopper makes it fit a test-tube of any size. The apparatus we employed was somewhat more complicated, for instead of heating the urine in a test-tube directly over a flame, we suspended the test-tube in which it was contained in a beaker of water over a lamp, so that the urine was thus very equally and gradually warmed, and the temperature of coagulation exactly ascertained.

General Results.—The general results at which we have arrived from the observations detailed in this paper are: That there are various albuminous bodies which appear in the urine. Some of these are derived from the digestive canal, and others from the blood, of which they form ordinary constituents. Those derived from the digestive canal may be either albuminous substances absorbed without undergoing digestion, as, for example, the white of raw eggs, soluble albuminous substances produced from coagulated albuminous bodies by incomplete digestion, or peptones. Those albuminous bodies which form constituents of the blood, and which we have noticed in the urine, are paraglobulin and serum-albumin. The former is in comparatively small quantity, the latter forming the great bulk of the albumin in ordinary albuminuria. Glycogen was only doubtfully present. The co-existence of sugar and albumin, which we noticed in one case, is interesting, as Bernard found that while puncture of one point in the fourth ventricle caused sugar to appear in the urine, puncture of an adjoining point caused albumin to appear; and Pavy succeeded by one puncture in causing them both to appear together. The effect of food is to increase the quantity of albumin in the urine, or even make it appear when it is absent during fasting. Its effect on the coagulating point is not constant, although it generally lowers it. This may be due to the fact, which we have observed, that while the earlier products of the pancreative digestion of fibrin have a lower coagulating point, the latter products have one which is higher than that of serum-albumin. In connection with this point it is interesting to notice that while pepsin occurs normally in the urine, we failed to find it, possibly because we operated on too small a quantity, and nevertheless we obtained evidence of the presence of pancreatic ferment

(trypsin). This is the first case we know in which this ferment has been found in the urine, and its loss in unusual quantity may possibly prove injurious to digestion, and be one cause why the albuminous products of imperfect digestion appear in the urine. Further observations are wanted, but this would seem to point to the existence of a vicious circle in this, as in other diseases, the albuminuria leading to the excretion of pancreatic ferment, and the loss of pancreatic ferment leading to imperfect digestion with excretion and loss of its soluble albuminous products. In a case recorded by one of us elsewhere, pancreatine was found to lessen the albumin, or even remove it entirely from the urine. We also found a diastatic ferment which might either be ptyalin, already found by Cohnheim in normal urine, or the diastatic ferment of the pancreas. Intestinal ferment converting cane into grape sugar was doubtfully present in the urine.

It is certain that albuminuria cannot be removed, like diabetes, from its place amongst diseases of the kidney to be classed entirely with diseases of assimilation, but probably some cases are almost or entirely due to disordered assimilation, and also in those cases where the kidney is undoubtedly diseased, the loss of albumin is increased by disordered assimilation. Attention to this point in the pathology of the disease will of course have an important bearing on treatment, and while the effects of remedies as tried in a case of confirmed Bright's disease, such as Gollop's, have been hitherto unsatisfactory, it may yet be possible, under the guidance of a more correct pathology, to attain to more efficient measures of cure. The coagulating point of the albumin in urine may be readily ascertained without trouble at the bedside. The indications which it gives are not decisive as to the nature of the disease, but more extended observations than we have been able to make may, and probably will, yield much information useful both in prognosis and treatment.—*St. Bartholomew's Hospital Reports, vol.* xiii., 1877, *p.* 283.

26.—ON SOME POINTS CONNECTED WITH DIABETES.

By Dr. F. W. Pavy, F.R.S., Physician to Guy's Hospital.

From time immemorial diabetes has been one of the most inscrutable of diseases. All sorts of vague notions have existed regarding the nature of the affection, and at the present day it must be said that opinions are by no means settled upon the fundamental points to be dealt with. As a close worker for the last twenty-five years, as my time has permitted, upon the subject, I have endeavoured by investigation to ascertain the true state existing, and I propose here to set forth the facts and

arguments which appear to me to lead up to and justify certain definite conclusions.

First, let me give a general expression of the outward evidence manifested to us on viewing the diabetic as compared with the natural state.

In the healthy person, when starch and sugar—principles which form important elements of our food—are ingested, they become lost sight of in the system. We know that the starch is first converted into sugar, so that the two are brought to the same position before absorption occurs. The sugar thus entering the system is subsequently so acted upon as to be rendered susceptible of being applied to the requirements of the economy. The healthy system, it may be said, as the result of observation, possesses a power of assimilating and subsequently utilising the absorbed saccharine matter, and hence we do not find it escaping in an unconsumed state from the body. In diabetes, on the other hand, there exists a want of assimilative power over the saccharine principle. From this defect, the starch and sugar of the food escape, under the form of sugar, or in an unconsumed state, with the urine. Observation shows that in proportion to the starch and sugar ingested sugar is eliminated by the kidney, besides a certain amount taking its origin in another way. We have here a simple statement of fact without the introduction of any theory. In the one case the sugar disappears from view, and doubtless, as a final issue, contributes to force-production; in the other it fails to be so utilised, but passes through the system and escapes in an unconsumed state. This constitutes an essential feature of the disease, and one great object of investigation is to find out, by following the internal changes, the explanation of this result.

I believe it is only through physiology that we can expect to be able successfully to approach pathology with reference to this subject, and we must be quite sure of our ground, step by step, as we advance. We must have correct information of the natural state before we can know where we are in relation to the unnatural. As the urine gives us the outward manifestation of the effects of the disease, let us commence our investigation by ascertaining what is to be learnt regarding this secretion in relation to sugar.

Of the tests for sugar, the copper solution (Fehling's, or a modification of it, the potassic-tartrate of copper, which I am in the habit of employing myself) is the most delicate and reliable. Now, on testing a sample of urine with this liquid, the reaction may be such as to leave no doubt that sugar is present to a more or less notable extent. With another sample of urine there may be no neat and decided reaction perceptible, but a certain amount of change suggestive of the indication of a slight

amount of sugar, although it must be said that this behaviour might be due to the action of lithic acid, especially if the urine were loaded with lithates, for lithic acid is an agent which has some reducing power over the oxide of copper. In another case, a sample of urine may be tested, and no sign of reaction shall be visible.

What is the true condition of these latter samples of urine? Are we to take the doubtful and negative behaviour for what they appear to represent, or may sugar be present, but fail to be revealed, or distinctly revealed, by our reagent? It is important, not, it is true, with reference to any clinical bearing, but as a physiological, and thence pathological, consideration, that we should know whether healthy urine is really free from sugar or not, and I will proceed to show what evidence is adducible upon this point.

It is known that our test agent which has been referred to does not act with the same sensitiveness in the presence of urinary matter as with a pure solution of sugar. Seegen has fully pointed this out. By this authority it has been shown that a certain minute quantity of sugar, which, when dissolved in urine, may fail to be susceptible of being satisfactorily revealed, may, when dissolved in water, be seen to give a distinct reaction. Hence we have here proof that a negative behaviour of our test is not to be taken as evidence of an entire absence of sugar.

Brücke some years back proposed a process for the separation of sugar by throwing it down in combination with oxide of lead. The process is an exceedingly satisfactory one, and by its means the sugar, to however minute an extent it may be present, can be abstracted, and thus, whatever exists in a large quantity of urine, may be obtained in a separated and concentrated form. It is just the process that is wanted for showing the condition of ordinary urine in relation to sugar; and not only may it be used for qualitative, but likewise for quantitative, purposes.

A few words will suffice to describe its mode of application, and in the course of my description I will illustrate the steps of the operation. The sample of urine, in any quantity that it may be considered advisable to take, is first treated with an excess of the neutral acetate of lead. The effect of this is to produce a copious precipitate, the uric acid, sulphuric acid, phosphoric acid, chlorine, and doubtless some other constituents of the urine, being carried down. The sugar remains in solution untouched. Filtration is performed, and the filtrate, which is the product with which we are concerned, is treated with ammonia and a further quantity of acetate of lead, unless a large excess of this agent has been in the first place used. The

sugar now falls amongst the copious precipitate again produced. In an acid solution—and such the urine as first taken was—the plumbic acetate does not lead to the precipitation of sugar, but in the presence of free ammonia a definite insoluble compound is formed, consisting of two atoms of sugar and three atoms of oxide of lead. The object now is to collect and wash the precipitate, and then liberate the sugar. It is quite requisite, in view of the subsequent application of the copper test, that all the free ammonia should be removed, and the washing is best effected by a few repetitions of subsidence and decantation before throwing on the filter. The washing is then carried on till the water which passes through no longer gives a blue colour to reddened litmus-paper. The necessity of removing all the ammonia has been mentioned, for unless this is thoroughly done, its presence will interfere with the proper precipitation of the suboxide should the copper test be subsequently employed.

Washing with hot water expedites the process by causing the precipitate to assume a more dense form, and thus more rapidly to subside, in the process by decantation, and also afterwards to be more readily percolated on the filter. Experience has taught me, however, that danger is incurred of losing some of the sugar. The combination between the sugar and plumbic oxide seems not to be a strong one, and with boiling water I have reason to think that the sugar may be entirely removed.

The washed precipitate is next treated in such a manner as to liberate the sugar. This may be done by the agency of hydrochloric, sulphuric, or oxalic acid, but a somewhat coloured product is the result. It might be suggested also that the acid may lead to the formation of sugar, or of a substance that, like it, exerts a reducing action on the copper test, from some other constituent of the urine, whether colouring matter or not, carried down with the lead precipitate. Schunk has pointed out, and I can confirm his statement, that boiling hydrochloric acid certainly appears to act in this way. No exception, however, can be taken to the use of sulphuretted hydrogen; and with this agent a better, because purer and more colourless, product is obtained. The precipitate with a little water is placed in a suitable apparatus and a stream of sulphuretted hydrogen passed through till decomposition is thoroughly effected, which may be known by the uniform production of intense black sulphide. With a moderate amount of precipitate a few hours will suffice for the purpose. Filtration is next performed, and the excess of sulphuretted hydrogen expelled by heat. The liquid is then brought down to a small bulk, either over the water bath or in the vacuum of an air-pump.

I have here mentioned the main points requiring to be referred to. Full details upon the whole subject I gave in a communication "On the Recognition of Sugar in Healthy Urine," inserted in the Guy's Hospital Reports for 1876.

If we take the product that has been obtained, and test it with the copper test a neat reaction is produced [result shown]. Such is the result that we notice, but the question arises—Is this result to be taken as conclusive evidence that sugar is present? There can be no fallacy arising through the presence of lithic acid, which exerts a certain degree of reducing effect with the copper solution, for this principle was eliminated with the first lead precipitate in the process employed. We need not, however, rest with the action of one test only. Moore's or the liquor potassæ, and Böttger's or the bismuth test, equally give a good reaction [result shown]. Further, there is another test, which is accepted as affording crucial evidence regarding the presence of sugar—I mean fermentation, and this may be shown to give a positive behaviour. In my early efforts I failed to obtain a reaction with this test, even although I had manipulated with urine to which I had purposely added sugar. I afterwards found the failure of reaction arose from the acidity of the product yielded by the process of preparation employed. It did not occur to me at first to look to this point, but Pasteur has shown that acidity opposes, whilst alkalinity favours, the occurrence of fermentation. After removing the acidity by neutralising with carbonate of soda, I found that fermentation actively proceeded. Some of the product obtained from healthy urine has been set going with washed yeast in this fermentation apparatus which I have devised for the application of the test. It may be seen that a considerable amount of gas has been generated and is accumulated in the upper tube. I propose to show, by the action of potash, that this gas consists of carbonic acid, and the chromic acid test will reveal the presence of alcohol in a few distilled drops from the fermented contents of the lower tube [experiment shown]. A counterpart application of the test with yeast and water was started at the same time as the other, and the result shows that only a comparatively insignificant amount of gas has been generated.

The evidence, then, that has been presented ocularly before you, may be taken, I consider, as affording a demonstration of the existence of sugar in healthy urine. It is confirmatory of the results obtained several years back by Brücke and Bence Jones. We are not correct, therefore, in inferring from a negative reaction under the ordinary mode of testing that proof has been afforded of an absence of sugar. It is customary, it is true, on examining urine and finding that no reaction is given by the copper test, to speak of it as free from sugar. As far

as the requirements of clinical observation are concerned, such language may continue to be used, for the amount of sugar which is beyond the reach of ordinary testing in a decided manner to reveal has no clinical significance belonging to it. This I need hardly say should be taken as thoroughly understood. When precision, however, has to be considered, we are not justified in so expressing ourselves.

Not only can it thus be said that sugar exists in healthy urine, but the amount present is susceptible of being expressed in definite terms. Having satisfied ourselves by the corroborative evidence of the fermentation and other tests that we have really sugar to deal with, the information afforded by the copper solution may be accepted for quantitative purposes. There is no difficulty in obtaining a satisfactory result with a moderate quantity of urine. It is only lately that I have realised this to be the case. Even 100 cc. (about four fluid ounces) will suffice for the analysis. With double this quantity it is even easy to obtain a concentrated product, which gives a very perceptible reaction on being tested in the ordinary manner with the copper solution.

For the quantitative analysis the lead process which has been described is employed to furnish the product for making the determination. To the quantity of urine named an ounce of a saturated solution of acetate of lead is added. Filtration is performed, and to the filtrate ammonia is added till it no longer produces a precipitate. The amount of acetate of lead employed not only suffices for the precipitation of the lithic acid, &c., but enough remains afterwards to carry down the sugar in the second precipitation with the ammonia. This precipitate is collected and washed, and then decomposed with sulphuretted hydrogen. For the determination of the sugar, the gravimetric process with the copper liquid—which I shall have especially to speak of further on—is employed.

The following are representations of the amount of sugar given with specimens of urine which, tested ordinarily with the copper solution, afforded the reaction specified in the table. It is convenient to represent the sugar in parts per 1000 for the sake of uniformity with the results that will hereafter be given relative to the blood and liver:—

Sugar contained in normal urine.

	Sugar per 1000 parts.
Urine from healthy person, giving no reaction when tested ordinarily	0·276
Ditto ditto	0·206
Ditto ditto	0·096
Urine from patient with tubercular meningitis	0·232

	Sugar per 1000 parts.
Urine from phthisical patient, giving trace of reaction, tested ordinarily	0·433
Urine from dyspeptic patient; very slight reaction, tested ordinarily	0·533
Urine passed after the administration of chloroform; decided reduction of copper solution, tested ordinarily	1·429

In order that the bearing of these figures may be realised, I will furnish upon the same scale the amount of sugar that would be contained in samples of diabetic urine of different degrees of intensity:—

Sugar in diabetic urine.

	Sugar per 1000 parts.
Urine of sp. gr. 1040, with 40 grains of sugar to the fluid ounce (a common condition in a severe case uncontrolled by diet)	87·90
Sp. gr. 1040, and 30 grains to the fluid ounce	65·92
Sp. gr. 1035, and 20 grains to the fluid ounce	44·16
Sp. gr. 1025, and 10 grains to the fluid ounce	22·27

The fact, then, that we have to deal with is that there is no abrupt line of demarcation or distinction of an absolute kind between the urine of health and that of diabetes. There is a difference, and a very marked one, it is true, from a quantitative point of view, at the two extremes, but the transition is not abrupt from the one state to the other. Every grade of variation in the amount of sugar is encountered upon different occasions. A sufficiency to give a slight reaction under ordinary testing is not uncommon, and sometimes an amount exists that can be definitely determined without the aid of precipitation by the lead process. I have, from time to time, come across specimens of urine that have contained a few grains to the fluid ounce—say from five to eight parts per thousand—as an incidental occurrence. These instances occasionally happen without the urine, before or afterwards, presenting a similar state, and without anything to point to the existence of diabetes. Such a condition has no clinical significance, and it is only where sugar persistently exists, or appears in the urine under the consumption of an ordinary amount of starchy and saccharine food, that it can be considered that a distinctly morbid state prevails.—*Lancet, March* 30, 1878, *p* 447.

27.—SUCCESSFUL TREATMENT OF DIABETES INSIPIDUS.

By Dr. HENRY KENNEDY, A.B., Physician to Simpson's Hospital, and the Whitworth, Drumcondra, &c., Dublin.

Whilst lately reading a lecture by Trousseau, on polytipsia,

I was struck with the gloomy picture he gives of the affection, both as regards its termination and treatment. Through the entire lecture indeed the idea is conveyed, that, if not an incurable, it is at least a most intractable disease; and towards the end of it he goes so far as to state that the saccharine diabetes is more hopeful even than the diabetes insipidus. Nor are these views confined to Trousseau alone. In a very able and elaborate work by Dickinson, just published, similar views would seem to be held; and also in the volume of Ziemssen's Cyclopædia which has lately appeared.

Now such experience as I myself have had is strictly opposed to this teaching, and I hope to prove it erroneous before this paper is finished; at any rate, the question is of sufficient consequence to bring under the notice of the profession in the pages of the Practitioner.

Before making any remarks, I will proceed to give a few cases of this disease, and the treatment I found successful.

Case 1.—A gentleman of forty-five years of age had followed the arduous duties of a general practitioner in England for upwards of twenty years. A legacy then left him made him easy in his circumstances, and he retired from practice, and on his arrival in this country came under my care. He told me that during this entire period, and even some years previously, he had a morbid thirst, and that he never drank less than four quarts of water in the day, and that it often was more. He attributed the attack in the first instance to cold. He took little or no strong drink, as he fancied it made him worse. The urine passed seemed to be in proportion to the fluid swallowed. It had no trace of either albumen or sugar, and its specific gravity was 1011. It was very light in colour, and had a very faint greenish hue. I told him I had read of cases like his being cured, and if he pleased I would try what could be done: and I spoke in this way, as this was the first case of the kind I had met. He was ordered one drachm of dilute nitric acid in a quart of water, to be used daily. I confess I was much and agreeably surprised to find that literally in the course of eleven days, this gentleman was cured, and though he had subsequently other disease, the morbid thirst never returned. I must say I was not prepared for such a result, and by such simple means; nor could I doubt that it was the acid had effected the cure. The patient himself, a medical man, also confirmed this, stating again and again, that he was being cured by the acid. It must be admitted that the case, though it had lasted for nearly thirty years, was not a severe one; for though the patient did not enjoy robust health, he was still able to perform duties which we know to be of a very arduous kind.

Case 2.—A lady, aged thirty-eight, married and a mother, had been getting into bad health for a considerable period before my seeing her. I found it difficult to ascertain how long; but it must have been the greater part of a year. Her symptoms were chiefly referred to the stomach and liver, and these certainly preceded the morbid thirst, which had gradually sprung up, and for which, in particular, I was consulted. My first impression of the case was that it was diabetes mellitus with which I had to deal. The tongue was rough and furred, and her mouth clammy. Her face was sallow, and wasted, and she had lost flesh considerably. There was great depression, and a feeling as if she would die, and the skin was harsh. Her description of the thirst was that she drank forty times a day, and she passed water in proportion to what she drank. Circumstances prevented me ascertaining the state of the urine at my first visit, and when I saw her a few days later I found the specific gravity was 1018, and that it afforded no evidence of either sugar or albumen. It had a shade higher colour too than in the case first given; and the smell was not characteristic of the presence of sugar. On account of her sallow look, she was ordered a few doses of blue pill, and then the dilute nitric acid, in the dose of one drachm daily, was given. This was subsequently increased to two drachms in the day. It is enough to state that this most unpromising case got steadily well. At the end of twelve days she was better, and in five weeks she might be pronounced well, and remained so.

[Dr. Kennedy relates other three cases, and proceeds:]

Such is the experience I have had of this affection; and it seems to me enough has been advanced to prove that, in some cases at least, the disease is curable, and by very simple means. It will have been observed that none of the cases were children; such as others have met with. But this does not affect what has been brought forward by myself. Whether children affected with this morbid thirst would be as readily cured as the cases given I will not take on me to determine. But my impression is they would. I think too it may be assumed that, at any rate in some cases, the cause of the thirst cannot be due to organic disease, such as most writers who have spoken of it would seem to attribute it to. It would be difficult to allow that any organic disease of the brain could be so completely cured as occurred in the cases given: and I must express my own conviction that the diseased state of which I have been speaking is entirely functional; even though I might be puzzled to say in what that state consists. Hunger and thirst are not, in their ordinary states, diseased feelings; nor when they become exaggerated are we justified in setting them down to organic disease; even though the morbid feeling may have existed for years.

Whether any other of the mineral acids, such as sulphuric or muriatic, would be equally successful in the treatment of this affection I cannot say. But such is not impossible. The former from its well-known astringent quality might be most useful. The truth is, that, with all the attention which has been, and is now being given to therapeutics, one of its most important principles has been in a measure overlooked. I mean giving a medicine in such doses as will produce its physiological effects; if it have any. In recent times Harley has the credit of having drawn attention to this point in his very admirable work. But I need not tell my readers that it was well known and acted on a century ago; and made the men of that period consummate treaters of disease. And this leads me to speak more particularly of the acid brought under notice in this paper. That it is a most potent medicine admits of no doubt; and that its curative effects are, often at least, only seen when it is given in very full doses is equally certain. Had I not acted on this idea, it is certain I would have failed in some of the cases which have been detailed. It will be recollected that in three of these the doses respectively were two, four, and five drachms in the day. From these doses the specific effects of the acid were very shortly evidenced, and as these are not generally known, it will not be out of place to speak of them. About the fourth day the patients say they do not feel quite well; the appetite at the same time failing a little. If the pulse be felt, it will be observed to be fuller than usual, and headache may be complained of. But what is specially noticed is aching of the jaws and teeth, the patients supposing they have got toothache. But it differs in this respect from common toothache—that it affects both sides. If the gums be now examined they will be observed to be swollen and redder than natural; but though the state resembles the salivation from mercury, I have not seen any ulceration. In two of the cases this state became so marked and the suffering so great, that I was obliged to suspend the use of the acid: and it was very remarkable how rapidly the unpleasant effects then subsided. Two days were sufficient for the purpose; and the fever, for such it was, which the acid had raised in the system, had very nearly disappeared. In this respect there is a very marked contrast between the effects of the nitric acid and mercury; for we know the effects of the latter are much more enduring and depressive. This power of raising a kind of fever in the system, I take to be of great value in therapeutics. Nor is it confined to the medicines named. Iron will cause it, and, if I mistake not, so will arsenic. But probably few will cause it as rapidly and safely as the acid I have brought under notice. Whether this medicine may be found of value in what I cannot but consider the infinitely more serious

affection, diabetes mellitus, I cannot take on me to say. In one case where I gave it, it did no good. The patient soon complained of griping, and it had to be given up. In this instance, however, the disease was very far advanced. I can easily understand that at an earlier period, and more particularly if it were an object to lessen the thirst, so often present, the nitric acid might prove very useful.—*Practitioner*, *Feb.* 1878, *p.* 94.

28.—ON CASTS OF THE URINIFEROUS TUBES.

By Dr. James Sawyer, Prof. of Pathology in Queen's College, and Physician to the Hospital, Birmingham.

First in importance among organic urinary deposits are tube-casts, moulds of the uriniferous tubes of the kidney, found in the albuminous urine of persons suffering from various renal diseases. You must learn to distinguish the different varieties of these casts, together with the pathological changes of which they are indications. An accurate knowledge of these deposits is necessary for the exact diagnosis of many affections of the kidneys, and without such knowledge no reliable prognosis can be readily given. The urine-tubes of the healthy kidney are about 1-700th of an inch in diameter; they are lined by nucleated epithelial cells, of spheroidal shape, and lying in a single layer upon a basement membrane. The nucleus is round, and its diameter varies from 1-4,000th to 1-3,000th of an inch; it is surrounded by finely-granular matter, and in the convoluted tubes a distinct cell-wall cannot always be seen. The nucleus may be perfectly free. In the straight tubes the epithelium is flatter and more squamous than in the convoluted tubes, and the cell-wall is very distinctly marked. Renal epithelium is only found in urine as a product of disease.

I advise you to learn Dr. Beale's definition of a tube-cast:—"A cast consists of a mould of a uriniferous tube, and is composed of some transparent material which is formed in, or poured out into, the canal, and there rendered firm, entangling in its meshes whatever may be in the tube at the time of its effusion." Dr. Johnson has classified casts as follows:—Epithelial casts; large waxy casts; small waxy casts; granular casts; oily casts; bloody casts; purulent casts.

Epithelial Casts.—Epithelial casts are also called desquamative casts, and consist of tubes or moulds of coagulable matter, exhibiting epithelial cells contained or entangled in, or adherent to, the transparent exudation. The epithelial cells are rarely perfectly normal in appearance; while maintaining their general characteristics, they are usually more or less disintegrated. These casts are of medium size as compared with other sorts of casts, and measure about 1-700th of an inch

in diameter. "Opaque granular casts" contain disintegrated epithelium.

Waxy Casts.—These casts vary much in size, the smaller ones having a diameter of about 1-1000th of an inch. They are clear, transparent, glistening cylinders, and they may be invisible until tinted by means of iodine. Sometimes they may be slightly granular, especially the larger ones, and they occasionally contain a few epithelial cells. Waxy casts have also been named hyaline casts. Fatty and epithelial casts may co-exist in the same urine with waxy casts.

Fatty Casts.—These casts appear as tubes more or less completely filled by minute oil globules. They sometimes include epithelial cells, presenting the marks of fatty disintegration.

Besides these chief varieties of renal casts, tube-casts may be seen filled with blood corpuscles, or with pus cells—bloody or purulent casts.

Urinary deposits which are made up of renal epithelium and tube-casts generally appear cloudy or flocculent to the naked eye.

Speaking generally, I may affirm that tube-casts indicate organic disease of the kidney, and that the various forms of casts have different significations, both as regards diagnosis and prognosis. I may sum up the particular indications of the several sorts of casts as follows:—Epithelial casts and blood casts mark acute disease; granular casts indicate sub-acute or chronic disease; waxy casts characterise chronic disease, leading to serious textural changes in the kidney; fatty casts, frequently found in conjunction with the waxy, result from fatty degeneration of the renal epithelium; pus casts have been found in cases of multiple abscesses in the kidney. You must never draw diagnostic inferences from the appearances of a small number of casts; your conclusions must be based, after repeated examination of various specimens of urine, upon the general characters of the prevailing types of casts seen in the deposit. But any conclusions founded upon the microscopical evidences of renal disorder must be compared with and checked by the diagnostic indications to be derived from a study of the history and condition of each individual case of disease.—*Med. Examiner, Nov.* 1877 *p.* 880.

SURGERY.

FRACTURES, DISLOCATIONS, AND DISEASES OF THE BONES AND JOINTS, &c.

29.—ANTISEPTIC SURGERY.

By S. Messenger Bradley, Esq., Surgeon to the Manchester Royal Infirmary, and Lecturer on Practical Surgery at Owens College.

There are two theories by which putrefaction is explained: the chemical theory and the germ-theory. The chemical theory is thus described by Neudörfer, from whose recent work, Die chirurgische Behandlung der Wunden, I make the following free translation. "Such albuminous bodies as blood, serum, and lymph possess a very unstable chemical constitution, and are only capable of resisting change whilst contained within their proper vessels. As soon as these boundaries are passed, they undergo molecular change, fermentation, and decomposition. As excitors of these changes, we must regard:—1. All altered albuminous bodies which belong to the animal kingdom, since they have the property of inducing this molecular change and decomposition in unchanged albuminous bodies with which they come in contact; 2. Altered albuminous bodies met with in the vegetable kingdom, such as lint, cotton, charpie, and other surgical appliances; 3. Water, even distilled water, coming into contact with albuminous matter, induces the changes above referred to; 4. Mechanical causes, such as blows, &c.; 5. Dynamic causes, like pain, &c." (Page 154.) Such, shortly, is the chemical theory of putrefaction, concerning which I shall content myself with saying that this explanation is no explanation at all, since the changes referred to by Neudörfer never occur unless there is some means of access for the air. Keep out the air, and these unstable albuminous bodies never do putrefy. They may escape from their boundaries; they do escape every day in simple fractures and other subcutaneous injuries, yet we confidently predict that there will be no putrefaction; for we know that, without a *primum movens*, these fluids are incapable of change. Neudörfer himself admits as much when he speaks of these changes being excited by other albuminous bodies which have themselves already undergone a change. This fact led observers to examine the air, and to

inquire whether it was the air itself, or something in the air, which induced the putrefactive change; and ere long it was discovered that, if the air were carefully filtered, putrefaction never occurred. The search for this *tertium quid* led on to the detection and indictment of bacteria as the *vera causa* of putrefaction. This naturally leads me, then, to consider the second theory of which I spoke; viz., the germ-theory of putrefaction. It is true that as yet there is a link missing. We know, from the beautiful experiments of Tyndall, that air optically pure is incapable of producing putrefaction; we know that, wherever there is putrefaction, there are bacteria; but the origin of these organisms, the germs of the bacteria, are beyond our ken, and, from what I know of the matter, are likely to remain so. This, however, no more proves their non-existence than the apparent identity of embryological cells proves their similarity: the facts equally point to the conclusion that our vision, even aided with the microscope, is very limited.

Permit me to dwell for a few moments upon certain points in the life history of these little organisms. And, first, I would remark that, like the higher animals, they are a mixed lot; sheep and goats are to be found amongst them. Of them, it may be truly said, "By their fruits shall ye know them;" for, while some are innocuous to the human system, others are lethal, giving rise to various specific diseases. Already the microscope has done much towards branding the various kinds of bacteria; much more, however, remains to be done in this direction.

The important fact for us to bear in mind in connection with the matter in hand is this: that, while some are harmless and others are harmful, all alike are capable of producing putrefactive change. Such tiny things as bacteria are, of course, difficult bodies to study; but we have learnt some important principles of their mode of living: *inter alia*, we know that they multiply exceedingly; we know that they prefer swimming to flying; we know what will kill them and what will keep them alive, and, of course, it is the study of this last fact that has led to the elaboration of antiseptic surgery. Bacteria depend, like we do, for their sustenance upon oxygen; but, like us again, they can only bear a certain quantity; too much kills them, and, for this reason, ozone is a powerful germicide and true antiseptic. I will, with your permission, refer to an experiment with this body, which I made last year in company with Mr. Harrison.

On February 5th, 1876, we took these two bottles, each containing some turnip-water just passing into putrescence; through the one, a stream of ozone was passed; in the other, the air was left untouched. Through each cork, bent tubes

were inserted. These bottles are now before you. The fluid through which the ozone was passed is little changed; the fluid in the other is green and thick. I have little doubt that the turbidity of the ozonised solution would have been quite obviated, if, instead of corks, we had used plugs of cotton-wool, and so filtered the air which subsequently found ingress to our bottle. This experiment may, however, be taken as sufficient evidence of the antiseptic properties of ozone, and it is by virtue of their ozone-evolving properties that some agents employed in surgery have antiseptic powers; for example, Dr. Angus Smith informs me that all the essential oils give off ozone, and he has proved that they all possess antiputrescent properties. Of these, he tells me the oil of mustard, the oil of bitter almonds, and the oil of the eucalyptus globulus are the most powerful; and with these I intend to experiment. From recent investigation, indeed, it would appear that the eucalyptus globulus (I mean the tree itself) really merits its character of malaria-killer. May we not fairly infer that this is due to the ozone given off from its fragrant leaves? It has sometimes occurred to me that the sacred groves of Scripture had a similar value; if so, we may readily understand that the heathen's god would avenge the desecration of his temple, when these groves were destroyed, by sweeping as a pestilence over the land. Turpentine owes its antiseptic properties to a similar cause. I do not know whether this is true of tincture of benzoin, but think it very likely. So much for the effects of too much oxygen upon bacteria; destruction equally awaits them if they get too little; by the one process, they are killed; by the other, they are starved: *voilà la différence.* It is on this latter account that all hydrocarbons, such as the fixed oils, are antiseptic, and, therefore, good surgical dressings; likewise it is by starvation, I imagine, that the anhydrous system of dressing wounds secures such immunity from putrefaction.

Turning to germicides proper, we find many substances capable of killing bacteria; boracic acid, iodine, and carbolic acid are the most in vogue. I do not know, nor do I think that it is accurately ascertained, in what proportions these substances kill bacteria; suffice it to say that, from my own experiments, I differ from Lister in saying that the No. 20 solution of carbolic acid (*i.e.*, 5 per cent. of the acid) is "as certain death to them as the gas-flame."

Before passing on to criticise Lister's special method of dressing, I wish to mention one other phenomenon attending the process of putrefaction. I refer to the evolution of ammonia. This may be taken as constant. It is true that Dr. Angus Smith, who most kindly has given me much information upon the subject of putrefaction, informs me that the presence

of ammonia is not an absolute proof of putrefaction, although it comes very near it; on the other hand, he says, "its absence is to be regarded as proof positive of the absence of putrefaction." Be its origin what it may, we have these two capital facts apart from all theories: putrefaction is invariably associated with the presence of bacteria, "surgical" putrefaction is invariably accompanied by the evolution of ammonia; the microscope reveals to us the presence of the one, chemistry the presence of the other.

I am inclined to think we may go further, and say that, where we have ammonia, there we shall find bacteria; where we find bacteria, there we shall find ammonia; and, the contrary to these two propositions, where there is no ammonia there are no bacteria, and where no bacteria are present there is no ammonia. To place this entire statement, however, on the basis of proved problems, further research is needed.

I now desire to address myself more specially to that method of practising antiseptic surgery introduced by Lister, with the direct purpose of seeing if its claims to the title "antiseptic" are pre-eminent; and first I would inquire whether it is correct to say that this plan always defies putrefaction. It may not go for much to say that, in my hands, it has occasionally, like other plans, failed to do so; but it is a good deal to say that it fails in the hands of such a master of all the details as Mr. Lund, yet such is the case; I do not say often; I do not know whether often or seldom; but, even in his skilful and careful hands, Listerism will not always succeed in warding off putrefaction. Some little time ago, Mr. Lund undertook an operation for the radical cure of hernia. I looked upon this operation with interest, as affording an excellent proof of the value of Lister's method; and, having shortly before had a case of hernia, where I had removed a large quantity of omentum, and where putrefaction had followed, I was almost prepared to accept the superiority of Lister's plan on the score of success in this case alone. I stated as much beforehand to Mr. Whitehead, and asked Mr. Lund, at the time of operation, if he would kindly let me examine the discharges following the operation, to which request he at once acceded. The operation was a long and serious one, involving the removal of a considerable portion of omentum, and the stitching together of the pillars of the external abdominal ring. Mr. Lund himself dressed the wound on every subsequent occasion. I did not think it worth while to examine the discharge for the first week or so; but, about the eighth day, I was present at the dressing, and, at my request, Mr. Lund touched the microscope-slide I held ready with the end of the drainage-tube, which he drew from the wound. At this time, the incision had nearly healed,

and the wound looked very clean and healthy, though I thought there was some detectable fœtor about the pus; of this, however, it was difficult to speak, owing to the prevailing odour of carbolic acid volatilised in the spray. Under the microscope, the discharge was found to swarm with bacteria, and the patient died some days later of septicæmia. Now, gentlemen, although this is but a single case, it serves sufficiently my purpose to prove that there is no infallibility about Listerism. To admit that it does fail from time to time, even in the hands of so perfect a manipulator as Mr. Lund, is to admit that it must fail, and, therefore, its claims to our acceptance as the one true method must be based upon other grounds than its universal success.

To what, then, do we turn? Naturally, to statistics; but here we are met with a difficulty. Lister, himself, so far as I know, has not published his statistics, and so we must be content to compare the statistics of his followers with those of surgeons who dress on other plans. Well, gentlemen, I do not think that any marked superiority is to be made out for Lister on this score; *e.g.*, the results obtained by Callender are fairly equal to those of any follower of Lister. I would crave permission, by way of further information on this head, to quote my own statistics of amputations and excisions from January, 1876, up to the present date, during which time I have employed open dressings, according to the method shortly to be described.

During the period mentioned, I find that I have had fifty-five amputations and excisions, with the following results. I only quote cases under my care at the Infirmary.

	Recovered.	Died.	Total.
Amputations.			
Forearm	3	0	3
Elbow-joint	0	1	1
Arm	4	0	4
Shoulder-joint	1	0	1
Hip-joint	1	0	1
Thigh	12	3	15
Leg	2	0	2
Ankle	8	0	8
Excisions.			
Elbow	8	0	8
Hip	2	0	2
Knee	4	1	5
Wrist	2	0	2
Removal of loose cartilage from knee	1	0	1
Operation for ununited fracture of forearm	1	0	1
Total	50	5	55

The important question here arises, Were any of these five deaths attributable to putrefaction or its septic consequences? and to this query I confidently reply, No, not one. The death following amputation at the elbow-joint occurred on the second

day following the operation, and was distinctly due to shock, the patient, an old toper, never rallying. Of the three deaths following amputation of the thigh, one was the case of a woman, whose thigh I amputated, on account of gangrene sequential to typhoid fever, and in whose limb the artery was found blocked right up into the groin; the second was a case of diffuse aneurism, in which I had previously tied the common femoral and afterwards amputated; the man died of hemorrhage, but the stump had healed; the third was an old Irish woman aged 72, who made a good recovery up to the third week, the stump being nearly healed, when she received bad news from home and sank. The death following excision of the knee was in a little girl of two years of age. She became sufficiently convalescent from the operation to be sent to Cheadle; but I afterwards learned that she died there from, I believe, diathetic causes. Such are the statistics I have to submit to you; but you may say: Well, you have a fair percentage of recoveries; but have you not exposed your patients to greater risk than you would have done had you dressed them *à la* Lister? To this question, I scarcely know how to reply better than by submitting the temperature-charts of each patient; this, indeed, I hoped to do; but some have been lost and others I never received; what I have, however, I place before you, and I think I may say that, so far as they go, they afford no evidence of septicæmia, nor, I think, will you find amongst them one which shows an unnaturally high febrile condition as consequent upon any of these capital operations.

So much for the infallibility claim and the statistical question. I now come to positive objections to Lister's method, chief amongst which I place my objections to the spray. My objections to the spray are partly theoretical and partly practical. For some time past, I have thought it probable that the spray, instead of keeping off bacteria, really drove them into the wound; creating currents and vortices in the air, it seemed likely to me that the skirts of these little whirlwinds would be caught, and any contained micozymes entangled and driven along the lines of spray; for be it remembered the spray is not a continuous sheet of vapour, but only a good thick douche, with plenty of room for thousands of bacteria to dance between each and every atom. Many of you have seen, in the experiments with vibrating sand-plates, how the light lycopodium is caught and held in the very centre of the sites of greatest vibration, while the heavier sand is driven through into the lines of rest; similarly, I thought the bacteria might be trapped by the very means adopted to keep them at a distance.

Again, I thought the water mixed with the carbolic acid

might be a fertile source of mischief; it is true the solution used is a strong one; but, as I have before mentioned, I have reason to question Lister's dictum as to the fatal effect of this No. 20 solution, as it is called, upon the lives of bacteria. Over and over again, I have seen bacteria moving as vigorously as before, after two hours' immersion in this solution. Holding these opinions, I was anxious to test their accuracy, and, therefore, I performed the following experiment.

On October 20th, 1877—that is, six weeks ago—I cut off two pieces of muscle from the leg of a girl whose thigh I had just previously amputated. One piece was removed under the spray, placed upon this board, quite new, and wiped with a sponge soaked in No. 20 carbolic acid solution, and dressed strictly according to Lister's plan. The other piece of muscle was cut off in the common air and dressed with lint saturated with glycerine and carbolic acid, in the proportion of one part of glycerine to eleven parts of carbolic acid. From that date to this, my dresser (Mr. French) has taken charge of these specimens, and dressed them each on the method first adopted about every third or fourth day. Last Friday but one I saw them for the last time until to-night. I found the muscle dressed *à la* Lister whitish in colour, flabby, and soft; the other dark in colour, firm, dry, and semitransparent. I then Nesslerised both specimens, and found that the one dressed with the spray gave ammonia, and was therefore putrefactive, while the other gave no ammonia, and was therefore sound.

The following is the mode I myself employ:—It is unnecessary for me to say there is nothing original about my plan; it is necessary, however, that I describe it. In the first place, I thoroughly cleanse the skin with carbolic soap, even rubbing off the outer layer of cuticle, thus removing all clinging germs. I take care that all instruments, etc., are perfectly clean. After performing the operation, I fill the wound with some antiseptic, generally selecting No. 20 solution of carbolic acid, although many other preparations, such as chloride of zinc, or sulphate of iron, or nitrate of silver, seem to possess equal germicidal powers. I then take pains to render the wound as dry as possible, after which the sutures are introduced and the wound dressed with four or five folds of lint thoroughly well saturated in a mixture of carbolic acid and glycerine. Over all, I place either a pledget of carbolised tow, or dry lint, or cotton-wool. If the subsequent discharge be abundant, I at once liberate some of the sutures, to give free vent to the discharge, and have the wound dressed at least twice a day, otherwise the wound is dressed once a day. I carefully avoid using water in any shape or form, squeezing the matter out, not syringing it out, and wiping with dry, not with wet, lint. It will easily be understood that there are cases where the position of the pus, &c.,

renders it impossible to remove it altogether by pressure; this, for instance, is the case when the knee-joint, or indeed any joint except the hip-joint, is opened; then it becomes necessary to wash out the discharge with the syringe. When this has to be done, I am in the habit of using a solution of permanganate of potash in preference to carbolic acid, and take care that the syringing is thorough, and performed twice in the twenty-four hours, so as to give little time for decomposition to take place. Irrigation in this case is good; indeed, is excellent; but it is somewhat messy and involves some exposure to cold, which is not always desirable or safe. There are two other conditions where the syringe comes into play, which I may take this opportunity of alluding to; I refer to the treatment of sinuses and abscesses. Speaking of the first, and supposing that all dead bone, etc., is removed from the bottom of the sinus, and that the granulation-tissue round the orifice is removed with the knife or scissors, I believe the best plan to secure closure of the entire sinus is to thoroughly flush it every day with a mixture of tincture of iodine and water (one to seven). Abscesses are, in my opinion, much better and more successfully treated on Callender's plan of superdistension, especially if pressure be exercised afterwards, than by the method of Lister. This at least is my experience, which in this respect, I believe, is in accord with that of some of my colleagues. You, of course, will ask me what proof I have that such a plan as the one I have detailed merits the term antiseptic equally with Lister's, and my answer is: I have given you my statistics, which, I think, will bear comparison with those of his followers; but I chiefly rely upon experimental proof.

You will please to bear in mind the two mutually supporting evidences we have of putrefaction: the presence of bacteria and the exhalation of ammonia. I make no mention of such questionable evidence as the smell or colour of the discharge.

Submitting my cases to this double test, I believe I am entitled to say that I practise antiseptic surgery; *e.g.*, among several similar experiments, permit me to mention the following. A fortnight ago I took the first four operation cases in the female wards, and, with two of my dressers (Mr. Challinor and Mr. Pownall), examined the discharges by the double test of the microscope and Nessler's solution. The cases chanced to be an amputation of the thigh performed three weeks before, a carcinoma on the chest, a lipoma of the neck, and an excision of the elbow, all operated on ten days prior to the examination. Besides these, we examined a case of ligature of the subclavian artery with another of my dressers, Mr. Williams, in the men's wards, of a week's standing. In none of these were bacteria to be found, and in none was ammonia to be detected. But understand my position: I do not claim for this plan that it

will certainly prevent putrefaction; for I know that, in some cases, putrefaction does occur, and, in my opinion, do what we will, ever will occur from time to time. What I allege of it is, that it is just as efficient as the more costly and cumbersome plan of Lister; and, in my opinion, this is no small boon. To the army surgeon, toiling in such a terrible war as is now raging, the simpler method I advocate is surely incomparably better, inasmuch as it is practical, while the other is not. But even to the surgeon engaged in hospital and general practice, the advantage of simplifying Lister's plan is considerable; for I am daily more and more convinced that such matters as the mysterious spray, and the patent character of the dressings, act as a real hindrance to the comprehension and general adoption of the true principles of antiseptic surgery.—*British Medical Journal, Feb. 23rd,* 1878, *p.* 256.

30.—ANTISEPTIC SURGERY.

By Dr. M. Thomas, Royal Infirmary, Glasgow.

Mr. Messenger Bradley takes credit, through his statistics, for the method of antiseptic dressing he has used during the last two years, and claims for himself a success equal to that of any of Mr. Lister's followers. Happening to have finished my operation statistical table for last year, it occurred to me that a fair comparison might be made between his results and those of one of our surgeons who is a follower of Mr. Lister, and who faithfully carries out his plan of dressing even to its latest development. I therefore give his results for the same period that Mr. Bradley has given his—viz., from 1st January, 1876, to 31st December, 1877, tabulating them, so far as practicable, in the same manner, for the purpose of comparison, under the heading B. and F., the latter representing the Listerian mode of dressing.

Table.—*Amputations.*

	B.			F.		
	Recovered.	Died.	Total.	Recovered.	Died.	Total.
Forearm	3	0	3	1	0	1
Elbow-joint ...	0	1	1	0	0	0
Arm	4	0	4	6	0	6
Shoulder-joint ...	1	0	1	4	0	4
Hip-joint	1	0	1	1	0	1
Thigh	12	3	15	5	0	5
Both thighs ...	0	0	0	1	0	1
Knee-joint	0	0	0	6	1	7
Leg	2	0	2	5	0	5
Ankle	8	0	8	1	0	1
Total	31	4	35	30	1	31
Mortality per cent.	11.42			3.22		

A glance at these tables will show the great superiority, so far as results are concerned, of Mr. Lister's method over Mr. Bradley's. This superiority is further brought out if we subtract from F.'s table those operations which are proved to be the least serious of the major amputations—viz., the one amputation of the forearm and the other at the ankle; and if we reckon as two amputations the patient who had both his limbs lopped off, we have thirty of the most serious operations in surgery with only one death, or a mortality of 3·33 per cent. The same analysis of the operations of B. gives twenty-four similar serious amputations, with a mortality of 16·66 per cent. As Mr. Bradley has in a manner said he would stand or fall by his statistics, I apprehend that those I have put against his fairly and unequivocally prove that his mode of dressing is not quite so good as that of Mr. Lister: and though his object in endeavouring to discover a simpler and equally efficacious method of antiseptic dressing is commendable, it must be acknowledged that he has as yet failed to do so. There is no doubt that Mr. Lister's mode of dressing is, if anything, more expensive than the ordinary method, though this depends a good deal on how it is used; but the extra expense is not so great as to appal or prevent hospital surgeons from adopting it, if by doing so they can reduce the mortality of amputations from one in nine to one in thirty-three.—*British Medical Journal, March* 9, 1878, *p.* 336.

31.—ON THE ANTISEPTIC DRESSING OF WOUNDS.

By JOHN CHIENE, Esq., Assistant-Surgeon, Edinburgh Roya Infirmary.

Antiseptic surgery at present labours under two disadvantages: the expense of the dressings, and the constant necessity for using a spray producer.

These objections would be overcome by any one who believed in the necessity of preventing putrefaction. Unfortunately, the majority of the profession do not believe in this necessity; or if they believe in the necessity, do not believe that the methods recommended can prevent putrefaction. It is therefore self-evident that a decrease in the expenditure and a simplicity in the application may encourage the profession to give this method of treatment a trial; and after they have tried it and seen the results, they may then be led to believe the truth of the principle on which it is founded.

The object of this short communication is to lay before the Society certain considerations, which, if adopted, may, in the first place, lessen the expenditure; in the second place, do away with the constant necessity for the spray. These con-

siderations have occupied my attention for some years, and as opportunity offered in the Clinical Surgical Wards, I have by experiment tested their practical utility.

First, The Lessening of the Expenditure.—There can be no doubt whatever that a very great increase in the expenditure has lately taken place for surgical dressings. The dressing that is now almost universally used is Lister's Antiseptic Gauze. To decrease the expenditure, three methods are available: first, to cheapen the gauze dressing; second, to use a cheaper material than gauze; third, to use a more durable material than gauze. The cost price of the charged gauze in the Edinburgh Infirmary Wards is at present 2¼d. a yard; and it is difficult to see how the cheapening process can go on much further. In order to obtain a cheaper material than gauze I have during the last two years, along with Mr. Gunn, in the laboratory of the Edinburgh Infirmary, been experimenting with different varieties of paper. Paper impregnated with carbolic acid, and with salts of carbolic acid, has been tried upon wounds in the Clinical Wards; but at the present moment I cannot say that I have yet obtained a paper dressing as efficient and cheaper than the antiseptic gauze. I hope, however, to continue these experiments during the ensuing winter, and to lay the results before the Society at some future time. Along with these experiments, I have been using systematically, since 1875, sponges wrung out of 1 to 20 carbolic lotion, and applied over the deep dressing before the application of the outer dressing; by this means I have been enabled to lessen materially the quantity of gauze used at each dressing. I have further been enabled to dress the wounds less frequently than before. The sponges improve with use. If obtained at wholesale prices from dealers in sponges, and if small sponges are used, they can be obtained at a remarkably cheap rate. The authorities of the Royal Infirmary of this city obtained for me, for 3s. 6d., 60 small sponges weighing 1 lb. The smaller the sponges, the more easily they can be applied. These sponges may be stitched together, forming a layer; or they may be laid singly on the deep dressing, and held in position by the outer dressing. Before application, the carbolic lotion must be squeezed from the sponge. The sponge is applied practically dry. The channels in it by capillarity suck up the fluid discharges; and if a catgut or horse-hair drain is used, the sponge may be looked upon as a direct continuation of the catgut or horse-hair drain; or if an indiarubber drainage tube is used, the power of the sponge may be likened to the suction power of a syringe on drawing up the piston. It is evident, then, that the use of the sponge has other advantages besides decreasing the expenditure, and I would strongly recommend their systematic use in the antiseptic treatment of wounds. It is no

uncommon thing to find that the spongy layer, acting as a reservoir, is so saturated with the discharge that the external gauze dressing is little altered; and I have frequently, in large recent wounds, squeezed from the sponge from six to ten ounces of dark-coloured serum, which must of necessity, if the sponges had not been used, either have remained in the wound, causing tension, or have passed into the gauze dressing, necessitating its removal at an earlier period. The sponges not only decrease the expense, but they lessen the risk, and save time and trouble by reducing to a minimum the dressings of the wound. They have another manifest advantage in cases in which bleeding is feared; the resiliency of the sponges enables the surgeon to apply firm pressure without injury.

Second, Is there any way in which the surgeon may dress his wounds without the constant aid of the spray producer?—Mr. Lister long ago demonstrated that the spray is not required during the dressing of a superficial wound, as an ulcer. Can we in any way so alter the external conditions of our deep wounds that they will resemble a superficial wound? If this can be done, then the spray will not be required as long as these conditions are kept up. During the last two months I have attempted in several cases to comply with these conditions. My success has been such that I feel justified in stating the simple method adopted. The cases were a parotid tumour, an excision of an epitheliomatous tumour of the arm, an amputation of a great toe, and excision of the elbow-joint. In these cases a permanent deep dressing was applied on the day after the operation, and fixed in position either with a bandage or with some sticky material, such as Canada balsam, or a solution of guttapercha in chloroform. From the experience I have had in these cases, I am of opinion that if the dressing is so arranged as to be perfectly porous, and if an absorbable method of drainage is used, as catgut, it will not be necessary to remove the deep dressing until the wound is superficial. As long as the deep dressing is in position, the spray will not be required. All that is necessary is to remove the outer dressing when the discharge reaches its edges; to damp with carbolic lotion and salicylic paste the deep dressing, and to apply anew an external dressing. It must be remembered that the deep dressing has lost its antiseptic qualities, while it remains as long as it is covered by the outer dressing, perfectly aseptic. It must, therefore, be thoroughly damped with carbolic lotion whenever it is exposed to the atmosphere, in order to destroy any mischief that may have fallen upon it during the exposure, and in order to render it actively antiseptic, so that when the dry gauze dressing is applied over it, no mischief may pass from it through the deep dressing into the wound. The spray is used at the

operation and at the first dressing, and afterwards only when the deep dressing is removed. I have found, as yet, a gauze bandage the most suitable method of fixing the deep dressing on the limbs. This method is therefore available in all operations on the limbs. A bandage may also be used in many wounds of the trunk. In some, however, it cannot be satisfactorily applied, and some trustworthy adherent material has yet to be found which will fix accurately the edges of the deep dressing to the skin, leaving the centre of the dressing porous, so as to allow of the free escape of the discharges. This method has another advantage; it approaches more nearly to the perfection of healing by "scabbing," and the wound is not irritated by the carbolic spray when exposed by the usual method.

I am well aware of the imperfections which have yet to be overcome, but any considerations which have for their basis the lessening of the expense and simplicity in application, will, I believe, further the advance of the antiseptic system.—*Edin. Medical Journal, Dec.* 1877, *p.* 509.

32.—ANTISEPTIC TREATMENT OF CHRONIC BURSITIS.

By Dr. ROBERT ROXBURGH, late Resident Surgeon, Clinical Wards, Royal Infirmary, Edinburgh.

The following cases, which occurred in the practice of Mr. Cheine, while he lately had charge of the Clinical Surgical Wards of the Edinburgh Infirmary, illustrate the great advantage of catgut as the means of drainage in certain antiseptic wounds.

1.—J. R., a miner, æt. 18, was admitted, 10th Sept., 1877, with an enlargement of the bursa patellæ, associated with pain, particularly on movement. No distinct history was forthcoming, but he had only suffered pain and inconvenience for a week. The skin having been purified with strong carbolic lotion, a small incision was made with a tenotomy knife into the bursa under the spray, and about half an ounce of sero-sanguineous fluid was squeezed out. A few threads of fine carbolized catgut were then introduced with sinus forceps, and the usual gauze dressing was applied, the limb being bandaged to a posterior splint. The dressing was left untouched for ten days, during which the patient never complained of pain, and on its removal on the tenth day the bursal swelling was found to have entirely disappeared, the tiny wound was completely cicatrized, and the portion of the catgut skim outside the wound was lying on the skin just as it had been left, but cut off by the cicatrix at its point of exit, so that it could be rubbed away with the finger. The patient returned home cured.

2.—L. F., a pawnbroker's assistant, æt. 16, was admitted 13th Sept., 1877, with chronic bursitis of the knee, which she

declared she had only noticed a fortnight previously, and which during that time had been causing her considerable pain. She could assign no cause for the swelling.

The treatment was identical with that in the former case. The fluid was straw-coloured, and amounted to a few drachms. The dressing was allowed to remain for ten days, when on its removal the swelling was found to be gone, the wound healed, and the remains of the catgut drain lying loosely on the skin.

In both cases the discharge on the dressing consisted merely of a serous stain, without a trace of pus.

These cases are examples of an important surgical principle. The object aimed at was not obliteration of the sac, but simply and solely relief of tension within it. The epithelial cells lining the bursæ had acquired the habit of over-secretion—a habit set up in the first instance by injury from without, but now perpetuated by the mechanical irritation of distension. As in other analogous cases, a temporary abandonment of the habit was all that was necessary to insure a return of the cells to their normal condition. This was accomplished with the least possible disturbance of the parts, and with almost no pain to the patient, while the drain, being of an absorbable material, permitted a free exit for discharge, but did not stand in the way of healing.

It may be claimed for this method of treatment that it is more certain and more rapid than counter-irritation, and much less painful than free incision, or injection with iodine; while in simplicity, a feature rarely attributed to antiseptic surgery, it could hardly be surpassed. One dressing only was used, and the spray was required but once; for although it was used as a precautionary measure during the removal of the dressing, experience would probably warrant us in guaranteeing that in non-suppurative chronic cases, such as these, healing would be complete in the ten days allowed. It is scarcely necessary to note that an essential of success is the careful exclusion of fermentative mischief from without. Had putrefaction occurred, suppuration would in all probability have followed; the catgut would have been insufficient to drain away the dense viscid pus; accumulation within the sac would have taken place, necessitating free incision; and so the cure would have been rendered comparatively tedious and painful.—*Edinburgh Med. Journal, Dec.* 1877, *p.* 498.

33.—THE NEW ANTISEPTIC—THYMOL.

By the Editor of the Medical Times and Gazette.

A rival to carbolic acid has certainly been discovered in thymol, the essential ingredient of oil of thyme, which is pre-

pared either by treating the oil of thyme itself with a strong alkaline solution, skimming off the thymene and cymol, which separate and rise to the surface, and precipitating the thymol which remains in solution with hydrochloric acid; or else (and this appears to be its most common commercial source at present) by distilling the seeds of *Ptychotis ajowan*—an East Indian umbellifer, which contain from 5 to 6 per cent. of their weight of this body. Thymol was discovered, according to Lewin, in 1719, by Caspar Neumann. Lewin first pointed out the comparative harmlessness of thymol internally administered; the absence of digestive disturbance after taking it, and its effect in checking abnormal fermentation in the stomach.

At present it is as an external antiseptic that thymol claims the earnest attention of the followers of Lister. The success which has attended its introduction into Professor Volkmann's clinic at Halle, as described by his assistant, Dr. Hans Ranke, in No. 128 of Volkmann's Sammlung Klinischer Vorträge, (a) is striking in the extreme, and we propose here to bring before our readers the method employed and the results obtained. In the main the general features of Lister's antiseptic dressing were retained by Ranke, thymol being substituted for carbolic acid with the single exception of the ligatures used for arresting hemorrhage and for deep sutures, which were always made with carbolised catgut. Since thymol is not entirely soluble in water in the proportion of 1 to 1000, the following formula was, after the first few trials, exclusively used for antiseptic purposes:—Thymol, 1 gramme; alcohol, 10; glycerine, 20; water, 1000 grammes. This "thymol solution," as it may be called for brevity's sake, has no corrosive action on instruments immersed in it, and in this respect is superior to solutions of carbolic, and still more of salicylic acid. It causes, however, when sprayed over the hands of the operator, a lively sensation of burning, accompanied with redness of the skin; but otherwise has no irritant qualities. Anæsthesia of the skin and epidermic desquamation, both of which are liable to occur under the use of carbolic acid, were never once observed in the case of thymol, nor did it exert any irritant action on the respiratory organs. The gauze bandages used for Lister's dressing were composed of the following materials:—1000 parts of bleached gauze, 500 of spermaceti, 50 of resin, and 16 of thymol, spermaceti being substituted for paraffin as a non-irritant, its object, however, being the same—namely, to retard the evaporation of the somewhat volatile thymol. In these proportions the gauze is extremely soft and pliant, it can be accurately adapted to a wound, and "sucks up" (to use Dr. Ranke's own expression) "blood and the secretions of the wound like a sponge." Owing to the impregnation of its fibres

with spermaceti and resin, they are unable to absorb the fluid, and as the latter distributes itself only in the meshes of the tissue, the bandage retains its elasticity in a high degree, even when thoroughly soaked. This thymol-gauze was directly applied to the wound, no "protective" being necessary, owing to the non-irritant quality of the thymol. Between the seventh and eighth external layers a piece of guttapercha paper previously washed with thymol solution was inserted in place of the ordinary hat-lining, and the whole was firmly fixed to the body with a gauze roller soaked in thymol solution, and tightly drawn, so as to seal up the parts almost hermetically against the outer air. Under these conditions very little thymol evaporates, and even at the end of eight days a very strong smell of thyme is perceived on removing the bandage. The thymol-gauze should be kept in stock wrapped in parchment paper, which should only be opened at the moment of using. The bandage must be removed and renewed as often as the least trace of secretion reaches its surface; but this necessity arises very much less frequently than in the case of Lister's carbolic dressing. In no instance, however, was the same dressing allowed to remain unchanged more than eight days. On those parts of the body to which it was difficult to adapt the dressing, the edges of the bandage and any other apparently weak points were strengthened with strips of benzoic wool.

On the whole, from the summer of 1877 up to January 23, excluding a number of slight injuries and trifling operations, thymol had been used in fifty-nine operations in Volkmann's clinic with the most excellent results. In the first forty-one cases the secretion was serous in only eight, and purulent in two. In the remainder there was absolutely no secretion—that is to say, when the bandage was removed, the skin of the protected parts was found completely dry, and not a drop of liquid could be squeezed out of the layers of gauze. This first series of thymol dressings includes cases of amputation of the mamma, of the arm, of the foot by Chopart and Pirogoff's operations, three amputations of the leg, four excisions of the elbow, two radical operations for hernia, and seven radical operations for the cure of hydrocele by excision. The sixteen severe operations treated with thymol during January of the present year include, *inter alia*, a gun-shot wound of the knee-joint treated by drainage of the joint, a secondary amputation of the thigh, an excision of the hip, and also one of the knee-joint for scrofulous caries, and an excision of the shoulder in an old case of dislocation of the humerus complicated with fracture, in all of which the results obtained were equal to those of the first series. Lastly, we should mention the successful termination of

three ovariotomies performed by Professor Olshausen, and treated throughout on antiseptic principles by means of thymol dressings.

To sum up Dr. Ranke's observations on the use of thymol, we may say that nearly all the major operations of surgery have been treated by him successfully by the thymol modification of Lister's method; and although at present the introduction of thymol offers no hope of any relaxation of the minute attention to details which a successful carrying out of this method invariably necessitates, yet since the secretion from wounds treated by thymol is much less, and their rate of healing much quicker, than when carbolic acid is used, thymol deserves the preference over the latter, the results obtained with it (antiseptically considered) being, to say the least, equally good. An additional advantage of thymol over carbolic acid consists in its innocuous effects on the system at large, and in its non-irritant action on parts to which it is locally applied. Thus, on the one hand, permanent antiseptic irrigations with thymol solution (1 per 1000), which cannot be carried out with carbolic acid for any length of time, have been repeatedly and successfully used in Professor Volkmann's clinic; and, on the other, the redness of the skin, vesication, and eczema produced by carbolic acid dressings have entirely disappeared on the substitution of thymol for it.

At present one kilogramme of thymol costs, in the German market, sixty marks (£3), whereas carbolic acid costs a little more than three shillings per kilogramme, so that, at first sight, the expense of thymol dressing appears to be very great. If, however, as Dr. Ranke clearly shows, we take into account the reduction in the number of bandages rendered possible by the use of thymol, owing to the extremely small amount of secretion induced by the new antiseptic, the difference in price is much more than compensated for. Thus, to give a single example of the superiority of thymol, we may mention the fact that two cases of diffused ganglion of the palm, treated by incision, only required two changes of bandage instead of eight or ten, as they would under the ordinary Lister's treatment.

The internal use of thymol in various diseases has at present scarcely answered to the expectations which were formed of it. Experimented with on a large scale by Coghen of Cracow, it only relieved the symptoms in a case of chronic gastric catarrh accompanied with fermentation; whereas in a number of cases of acute and chronic gastric and intestinal catarrh, in intermittent fever, chronic cystitis, typhoid fever, pneumonia, pulmonary phthisis, and chronic bronchitis, it completely failed. As an antipyretic, in doses of two to four grammes (Baelz), its action is also far inferior to that of salicylic acid:

hence, for the present, at any rate, it is for its valuable antiseptic properties that thymol deserves to be attentively studied; and there can be no doubt that Dr. Ranke's experience offers every encouragement to antiseptic surgeons to introduce it largely into their practice.—*Medical Times and Gazette, March* 2, 1878, *p.* 227.

34.—THE NEW ANTISEPTIC THYMOL GAUZE.

Messrs. SQUIRE, of Oxford Street, write to us :—As many of your readers are no doubt interested in the new antiseptic thymol, and may wish to have some detailed information about it, its price, and the various forms in which its preparations are to be had here, we beg to inform you that, at the desire of Mr. Spencer Wells, we procured from Germany last January some of the thymol gauze prepared with spermaceti. We have supplied this gauze during the past month to several of Mr. Wells's patients, and, before ordering or preparing more, we were anxious to ascertain his opinion as to its utility. His reply (which we are permitted to send you) is to the effect that, "while appearing to be quite as trustworthy an antiseptic as carbolic gauze, it is free from the objections of stiffness, of irritating action of the skin, and of the disagreeable odour of the carbolic acid. It readily permits transudation of any fluid escaping from a wound, but does not appear to increase any secretion, nor to irritate the edges of a wound, nor the skin surrounding it. The wound, as a rule, does not require dressing until it is time to remove the sutures, and one more dressing is all that is required. A solution of 1 part of thymol in 1,000 of water appears to answer well for irrigation, for spray, and as an antiseptic bath for instruments and for sponges." A solution of thymol in water of a strength of 1 in 1,000 is all that is required. Thymol is soluble in warm-water in that proportion, and there is no separation on cooling.

As your readers may wish to know the price at which this gauze can be supplied, we may add that at present it is sold in packets, of six yards long by a yard wide, at 3s. 9d., or 8d. per single yard. When manufactured on a large scale, some reduction may possibly be effected. Thymol itself cannot be sold here for less than 2s. 6d. per ounce. Calvert's carbolic acid, No. 1, is 6d. per ounce; but if, as is expected, a solution of 1 thymol in 1,000 prove as efficacious as 1 carbolic acid in 40, the relative cost is in favour of thymol in the proportion of 5 to 1.

It may interest your readers to know that we have prepared for Mr. Wells an adhesive plaster, containing 1 part of thymol in 1,000 of plaster, which appears likely to fulfil the desire so

often expressed of a non-irritating antiseptic adhesive plaster.—*British Medical Journal, March* 9, 1878, *p*. 336.

35.—ON A CASE OF EXCISION OF THE KNEE-JOINT, AND ON HORSEHAIR AS A DRAIN FOR WOUNDS.

By JOSEPH LISTER, Esq., F.R.S., Professor of Clinical Surgery in King's College, London.

Next to the importance of the avoidance of putrefaction in wounds is the prevention of tension by providing a free escape for effused blood and serum. This we have hitherto generally done by means of the caoutchouc drainage-tube of Chassaignac. But in the present case such a tube would have been unsuitable, because the natural position for the drain was that it should run between the ends of the bones which, as we have seen, were pressed together, so that the calibre of a caoutchouc-tube would have been altogether obliterated, and the drain in a most important part of its course rendered useless. Under these circumstances I used a drain of horsehair, because such a drain operates by capillary attraction through the interstices between the hairs, and those interstices cannot be obliterated by pressure, seeing that the hairs are not individually compressible.

The drain was introduced in a manner which you will often find useful. It may frequently happen that the most dependent part of a wound may have no opening in the skin to correspond with it: thus after excision of the mamma it may turn out, when the operation is concluded, that the wound presents a pocket extending considerably further back than the outer angle of your incision. Under such circumstances it is desirable to make an opening for the exit of the drain at the most dependent part. Now, if this were done by a puncture with the knife, some arterial branch of considerable size might be wounded, involving the necessity of freely enlarging the wound to secure the bleeding point. But if you take a pair of dressing forceps, and bore steadily from within outwards, the conical extremity of the instrument will slip past any arterial branch or nervous trunk without injuring it, and when at length it is apparent that there is nothing but skin between the instrument and the surface, the tough integument is divided with a knife over the point of the forceps, and the blades being forcibly expanded so as to enlarge somewhat by laceration the opening which has been made in the muscles, or other deeper textures, the drain is seized between the blades of the forceps, and drawn into place. So in the present case the most eligible position for a dependent opening was at the outer aspect of the limb, where the use of a knife would have involved the risk of injuring the external popliteal nerve, or of dividing some articular arterial branch.

Any such difficulty was avoided by employing the dressing forceps in the manner described.

It is only right that I should mention, when alluding to the horsehair drain, that its use did not originate with myself. We were led to its adoption in the following manner. Mr. Chiene, of Edinburgh, suggested some time ago the employment of catgut as a substitute for the caoutchouc tube. He hoped by this means to provide adequate drainage through capillary attraction, and at the same time, by virtue of the proneness of the catgut to absorption, to do away with the necessity for the withdrawal of the drain from time to time, which there is when the caoutchouc tube is used, whether for the purpose of shortening the tube or substituting a small one for a large. Mr. Chiene's anticipations were to a considerable extent realised. In all cases in which the wound remained aseptic the absorption of the deeper part of the catgut drain, and consequent falling off of the part outside the wound, might be reckoned on as a matter of course; and in several cases in which the catgut was so used, both by Mr. Chiene and afterwards by myself, the drainage proved adequate and satisfactory. Mr. White, of the Nottingham General Infirmary, afterwards substituted horsehair for catgut; not because it was supposed to be superior, but because, whereas the prepared catgut is a somewhat expensive article, a horse's tail is a very cheap one. A notice of this use of horsehair was published by Mr. White's house-surgeon, Dr. L. W. Marshall, in the Lancet of Dec. 2nd, 1876; and in the following month it was employed by myself, in the Edinburgh Royal Infirmary, in a case of chronic bursitis of the sheaths of the flexor tendons at the wrist, in which it seemed likely to be peculiarly serviceable. In this affection the bursa is distended both above the wrist and in the palm, the cavities thus constituted being connected by a constricted passage under the annular ligament; and it is desirable that both the expanded parts should be opened to give exit to the fibrinous concretions which are generally present (varying in size from that of a millet-seed to that of a small bean), and, further, that drainage should be provided for effused serum, the operation being performed antiseptically, in order to avoid the very serious inflammatory disturbance and suppuration which are otherwise apt to occur. I had previously used the caoutchouc tube as a drain in such a case, but I found a difficulty from the liability of the tube to be compressed by the tendons. This might, I thought, be overcome by the use of the horsehair drain, which at the same time would, for this particular purpose, be superior to one of catgut, because the catgut would probably be absorbed before the necessity for drainage would be over. Accordingly I cut down above the wrist, making my way between the tendons of the flexor subli-

mis to the distended sheath of the flexor profundis, and, as soon as this was opened, passed in a large bullet-probe, somewhat curved, slipped it along under the annular ligament, and pressed it forcibly towards the palm, so as to perforate the palmar fascia while avoiding injury to the palmar arch, and, having divided the skin over the point of the probe, dilated the opening in the fascia with dressing forceps, and then passed into the eye of the probe a substantial drain of horsehair, which had been well purified by steeping in a 1 to 20 solution of carbolic acid, and withdrew the probe, leaving the horsehair drain in its track. The drain answered admirably, and presented the further great advantage that it could be reduced in bulk in accordance with the diminution of the serous discharge, by drawing out as many hairs as might be desired; and in the course of three weeks, the last portions of the drain having been withdrawn, the wound healed without the occurrence of suppuration from first to last.

While the horse-hair has the advantage over the catgut that it can be used when necessary over a longer period, it has, in some cases, the converse superiority that it can be not only reduced in bulk, but withdrawn altogether at an earlier period than is required for the absorption of the catgut; for the catgut, in process of organisation and absorption, becomes more or less incorporated with surrounding tissues through the medium of the cells of new formation which invade it, and, if an attempt is made to withdraw the drain in whole or in part, there will often occur inconvenient oozing of blood through the rupture of newly formed vessels. And if, on the other hand, the drain is left intact till the parts of the catgut within the wound are entirely absorbed, there remains a small granulating sore at the place of exit of the drain, which may retard for some days the complete healing of the wound. Further, the threads of the catgut, as they undergo organisation, are increased in bulk by the formation of the new cells, and their interstices are liable to be more or less choked, so as to interfere with effective drainage. The horsehairs, on the other hand, lie unchanged among the tissues, and their interstices remain to the last as effective as they were at the outset.

The next case in which I used the horse-hair drain was one which you yourselves witnessed—viz., that of transverse fracture of the patella, treated by laying open the joint, drilling the fragments obliquely, and tying them together by means of strong silver wire. Being apprehensive that blood and serum might be effused into the joint to such a degree as to produce inconvenient tension unless a free exit was provided, I resolved to introduce a drain at a dependent part of the articular cavity; but I feared that, if a caoutchouc tube was used, it might be

rendered inefficient by being compressed between the condyle of the femur and the neighbouring tissues. I therefore had recourse to the horsehair, introducing into the posterior and outer part of the joint a drain, about a quarter of an inch in thickness, by means of the dressing forceps employed as before described. It worked to admiration; for though there was, indeed, in the first twenty-four hours, a very copious sanguineo-serous effusion, as shown by the soaking of the antiseptic gauze, yet not the slightest swelling of the joint occurred, and, after nine days, the small remains of the drain, which had been previously reduced at successive periods, were withdrawn, to allow the puncture to close. The drain of horsehair was as pure and white as if it had been merely dipped in water; having been washed quite clean of the blood which first occupied its interstices by the colourless serum which, after the cessation of the original sanguineous effusion, had been the only discharge. I was so much impressed with the satisfactory working of the horsehair drain in that case that we have since employed it in preference to the caoutchouc tube in all our wounds, and have had good reason to be pleased with the change. (If it be necessary to reintroduce a horsehair drain, it is readily done by taking a wisp of hair of half the thickness required, bending it in the middle at a sharp angle over a probe, and tying a piece of carbolised silk round it close to the probe, on withdrawal of which the drain is left with a rounded end which passes readily into the interior of the wound.)

[Prof. Lister made the preceding remarks on drainage in a case of excision of the knee-joint, which we have not particularised, and concluded with the following interesting observations :]

Let me remind you of the various important matters which that ulcer has afforded the opportunity of demonstrating. First, you recollect how putrid the sore was at the outset, and how we succeeded in purifying it once for all by applying to the epidermis soaked with putrid discharge a strong watery solution (1 to 20) of carbolic acid, which has a special power of penetrating the epidermis, and to the granulations a solution of chloride of zinc (40 grains to an ounce) which experience has shown to have an energetic antiseptic effect upon foul granulations. That we did really purify the sore by this application was proved to you by the fact that, being afterwards dressed with lint containing boracic acid, which is the mildest of our antiseptics, with a piece of prepared oiled silk interposed between it and the granulations, to protect them from the antiseptic, mild as it was, and to ensure constant moisture of the surface, yet when dressed after an interval of a week, the oiled silk, instead of being putrid as it would have been in twenty-four hours under a piece of ordinary lint, had no odour except that

of oiled silk itself. The pus had remained free from putrefaction for that long period, though not directly acted on by an antiseptic at all.

You have also had demonstrated to you on that sore some very important truths regarding the properties of granulations. You saw me clip away with scissors a portion of the surface without occasioning the slightest pain to the patient, proving that the granulations constituted a protective layer destitute of sensibility.

Again, we made an accurate pattern of the ulcer in gutta-percha tissue, and on comparing it with the sore a week later we found that the pattern was already considerably larger than the granulating surface together with the cicatrising margin already forming round it. Thus you had ocular evidence of the truth that granulations have a tendency to shrink, this being one of the means by which sores are diminished in extent in the healing process.

You also observed how, when the ulcer was protected, as far as was in our power, from irritation, by excluding both putrefaction and the direct action of the antiseptic, the formation of the epidermic pellicle at the edge proceeded with a rapidity never seen under water dressing.

Lastly, how instructive was the result obtained by skin-grafting. You saw that whereas before this operation was performed cicatrisation took place only at the edge of the sore, a thin superficial layer of integument, involving little more than epidermis, having been removed with a sharp knife from the inner side of the arm, and the shaving having been cut up on the thumb-nail into small bits, which were placed in succession, with the raw surface downwards, on the granulations, the grafts so planted became each one a centre of epidermic growth on the sore. Thus was illustrated the general fact in pathology, that new structures formed in the repair of injuries are composed only of tissues similar to those in the immediate vicinity, and the equally fundamental fact in physiology, that severance of a part from connexion with the body is not followed by immediate loss of its vitality.

You remember also how, having sprinkled the granulating surface with a sufficient number of grafts, we placed upon the sore the remaining portion of the shaving, about as large as a fourpenny-piece, and this, as you afterwards saw, took root and adhered by its entire under-surface, thus teaching us two great truths. First, it showed that the surface of granulations, if thoroughly healthy, may unite not merely with granulations, but with a freshly-cut surface, combining, so to speak, union by second intention with union by first intention. And, in the second place, it afforded of itself conclusive evidence of a most

important pathological fact not yet universally recognised, that granulations have no inherent tendency to form pus; for, before sufficient time had elapsed to cause the death of the portion of integument as the result of its severance from vascular connexion with the rest of the body, all pus-formation from the granulations on which it was placed must have ceased; and not pus-formation only, but serous oozing also, which would have been equally incompatible with union of the two surfaces. No sooner did this piece of living dressing, perfectly unstimulating, chemically or mechanically, protect the granulations, than pus-formation and exudation of liquor sanguinis were alike suspended.—*Lancet, Jan.* 5, 1878, *p.* 6.

36.—ON THE TREATMENT OF HIP-JOINT DISEASE BY EXTENSION WITH MOTION.

By William Adams, Esq., Surgeon to the Great Northern Hospital, and to the Hospital for the Paralysed and Epileptic; Consulting Surgeon to the National Orthopædic Hospital.

The object of the present paper is to direct attention to the recent advances which have been made in the treatment of hip-joint disease by the American surgeons, to whom we are indebted for the discovery of two very important principles, and also for their practical application by means of most ingeniously contrived instruments.

The first principle is that of extension, as a means of relieving the most acute pain in joint-diseases, especially applicable to the knee and hip-joints.

The second principle is that of extension combined with motion during the progress of disease, the patient being allowed to walk about, so as to promote recovery with free motion in the joint, instead of the ordinary result of ankylosis obtained by long-continued rest and immobility.

There can be no doubt that the discovery and practical application of these two principles have completely revolutionised the treatment of joint-diseases, and changed our opinion with regard to the pathological conditions existing, especially as to the production of acute pain which, formerly was believed to depend upon acute inflammation, requiring active local, as well as general antiphlogistic treatment, such as leeches, blisters, calomel and opium, etc. It has now been proved to depend upon undue articular pressure and contact of inflamed surfaces, produced by reflex muscular contraction, and capable of relief by mechanical means alone, producing extension, whether this be applied by means of the weight and pulley, or by the screw and cog-wheel.

The object of extension is not, as generally supposed, to

separate articular surfaces, but to overcome reflex muscular contraction, and, by relaxing the muscular rigidity, to prevent undue pressure of inflamed articular surfaces, or their margins, when the joint is held in a flexed position by muscular contraction.

Extension, for the relief of pain, is applied by means of the weight-and-pulley apparatus whilst the patient is in bed, the extension always being made in the same direction as the long axis of the limb. It will vary, therefore, when first applied, as the leg may be flexed, adducted, or abducted; but as soon as the reflex muscular action has been overcome, the weight will be made to pull in the direct horizontal line. After the pain has subsided, the object with which the weight-extension is continued is to secure sufficient rest to the joint, and at the same time permit a limited amount of motion, as the patient varies his position in bed. It is not found, by experience, that the limited amount of motion thus permitted in any way interferes with or retards, the favourable progress of the case; but, on the contrary, this may be looked forward to with confidence in the great majority of cases after the pain has been relieved by the weight extension. The health of the patient also improves rapidly after the relief of pain.

The successful application of weight-extension as a remedy for pain, however acute it may be, in joint-disease, is the greatest discovery of modern times in the treatment of these affections. The originality of this discovery is claimed by Dr. Henry G. Davis of New York. In his work on Conservative Surgery, published in the year 1867, at page 206, Dr. Davis observes: "We consider it fully established that, when disease about a joint renders the movement of that joint painful, the joint is always liable to be destroyed by uninterrupted pressure effected through the contraction of the muscles passing over it." And again at page 212, "In diseases of joints, we were the first to point out, as an always present factor in their destruction, the existence of unremitting pressure, as effected by contraction of muscles passing over the joints, causing constant forcible apposition of the surfaces within the joint. This is a general principle appertaining not only to the hip, but to all joints similarly affected. The application of this universal principle should guide us in any joint-affection, whether the disease be internal or external, as soon as the disease renders the movements of the joint painful. When this fact was fully established in our mind, we were led to seek the best way of counteracting the contraction of the muscles, and soon came to the conclusion that a constantly acting force, however moderate, must eventually weary muscles by giving them no respite. We adopted the weight as applied by means of the

cord, pulley, and adhesive strips for this purpose, when the patient was confined to his bed or couch. This answered the indication perfectly, relieving all pain and constitutional disturbance, enabling the patient to enjoy his days free from pain, and to rest quietly at night, to relish his food, and to be nourished by it; in short, it robbed the disease of all its terrors."

Dr. Davis does not precisely state the period at which he arrived at the above conclusion as to the cause of the acute pain and its method of treatment; but it must have been anterior to the year 1855, which is the date affixed to his extension splint by the instrument-makers Otto and Reynders of New York. At page 308 he states: "In 1856, I had fully established the treatment of morbus coxarius upon the plan of overcoming the pressure effected by the muscles, through the means of *continued elastic extension*, and the mode with its results was very flatteringly commented upon by one of the editors of the American Medical Monthly of this city, in March, 1857."

There can be no doubt that interarticular pressure, or the pressure of inflamed articular surfaces—one surface being generally pressed against the margin of the opposite articular surface—must take place when the joint is drawn into a flexed position by the rigid contraction of the muscles, more or less of a spasmodic character, which always coexists with the period of acute pain. Mr. Hilton has particularly pointed out this fact in his Lectures on Rest and Pain, and has explained it to be the result of reflex nervous irritation, depending upon the anatomical fact of the various muscles surrounding the joint being supplied by branches of the same nerves, which are also distributed to the interior of the joint. The explanation of this period of acute pain in joint-disease, and its dependence upon interarticular pressure, produced by the abnormal contraction of the muscles surrounding the articulation, has been brought before the profession in a very able paper by Mr. Howard Marsh, in the St. Bartholomew's Hospital reports.

The English idea has always been rest and immobility to the joint. The American idea, during the last ten years, has been extension with motion, *i.e.*, preserving motion in the joint whilst the pain is relieved by extension.

In the treatment according to the English system, immobility of the joint is obtained by various instruments and splints; from that piece of surgical antiquity, the *long straight splint*, reaching from the axilla to the foot, necessitating the confinement of the patient in the horizontal position for many months, and many other contrivances, such as metal and leather splints to the joint, which permit the patient to move about on crutches,

to the now somewhat fashionable Thomas's splint, invented by Mr. Thomas of Liverpool, and described in his recently published work.

All these means succeed, more or less, in relieving pain and promoting recovery, although ankylosis is frequently produced, and this has generally been regarded as the most desirable termination; but in many cases they all fail in relieving pain, for want of the American extension principle, and also they do not prevent the occurrence of dislocation, or partial dislocation, the effect of which is to produce shortening of the limb with permanent lameness.

An example of this occurred recently in my own practice, dislocation, or partial dislocation, taking place whilst the patient was wearing a Thomas's splint. A young gentleman, aged 6, was brought to me in March of the present year, suffering from slight symptoms of hip-joint disease; no pain, but limping, with muscular rigidity, and slight flexion of the hip-joint. I ordered Thomas's splint, which was made by Mr. Krohne, and applied by myself, with his assistance. I also ordered an extension-apparatus to be used at the same time with Thomas's splint, as the absence of extension appeared to me to be its chief defect. The splint, however, appeared to answer its purpose so well, and the child could be so readily moved about and taken into the open air, and at the same time apparently improving, that the extension-apparatus was not used by the parents, and I saw no more of the patient until sent for in consequence of the occurrence of pain, chiefly at night. This took place nine weeks after the application of Thomas's splint, and it continued two or three weeks before I was sent for; and I then discovered that a dislocation, or partial dislocation, of the head of the femur had taken place; shortening of the limb was apparent, the top of the great trochanter being nearly on a level with the anterior superior spinous process of the ilium. After this the pain ceased, and the disease will probably be arrested; but there must remain permanent shortening, with lameness, and probably complete loss of motion. I am, however, now trying persistent extension night and day, in the hope of diminishing these evils, which could not have occurred if the American system of extension had been relied upon, or extension combined with Thomas's splint, which I ordered. On July 17th, Professor Sayre went with me to see this patient, as he was desirous of seeing cases in which Thomas's splint had been applied.

In my own practice, during the last ten years, since the publication of Dr. Davis's book, I have, in the treatment of hip-joint disease, relied chiefly upon weight-extension, at the commencement, continued day and night with the patient in bed,

especially if pain exist; but, as improvement takes place, I allow the patient to walk about with the assistance of crutches, wearing at the same time a firm leather splint moulded to the hip, reaching from the waist to the knee. This secures rest and immobility to the joint, whilst general exercise of the body is permitted; but during the day-time, whilst the patient is not walking about, the leather splint is removed, and the weight-extension applied whilst the patient is on the sofa. Immobility, therefore, is not long continued; and in all cases where the disease has been arrested in the early stage, after treatment lasting from one to two years, motion at the hip-joint has always been preserved, and no lameness whatever remains. Patients treated in this way some years ago are now able to walk and take horse exercise without any fear of the recurrence of pain or any inconvenience.

When disease has not been arrested in the first stage, but has advanced to abscess, with destruction of the joint, motion is generally lost, and ankylosis results; but in some cases, in which I have relied upon extension throughout the disease, an useful though limited amount of motion has resulted.

Before concluding this paper, I will allude to the second principle in the treatment of hip-joint disease, for which we are indebted to the American surgeons—viz., that of extension combined with motion, allowing the patient to walk about during the progress of disease, so as to promote recovery with free motion in the joint, instead of the ordinary result of ankylosis obtained by long continued rest and immobility.

The most recent American advance has been to combine extension with the preservation of motion, and at the same time to allow the patient to walk about without crutches, the weight of the body being transmitted to the ground by a steel instrument applied to the limb from the waist to the foot, or rather a little below the foot. Attached to the steel band round the waist, are two firm inelastic perineal bands, which, passing from before backwards below the tuberosity of the ischium on each side, receive the weight of the body. A raised boot of one inch in thickness is worn on the foot of the sound limb. The limb is connected with the foot-piece of the instrument by means of two long strips of plaster, extending from a little above the ankle to the middle of the thigh, and retained in position by a circular bandage. Attached to the lower extremity of each strip of plaster is a buckle, to which is fastened a strap coming upwards from the foot-piece of the instrument. When thus connected, extension is made by means of a rack and pinion in the longitudinal bar of the instrument at about the middle of the calf. Inversion and eversion of the foot can also be controlled by a circular cog-wheel placed a little below the hip-joint.

The instruments now generally in use in America for the application of extension with motion, whilst the patient is allowed to walk without crutches, are those invented by Dr. Taylor and Professor Sayre of New York, also one by Dr. J. C. Hutchinson, of Brooklyn, all of which are modifications of the instrument first invented for this purpose in the year 1855, by Dr. Henry Davis, of New York.

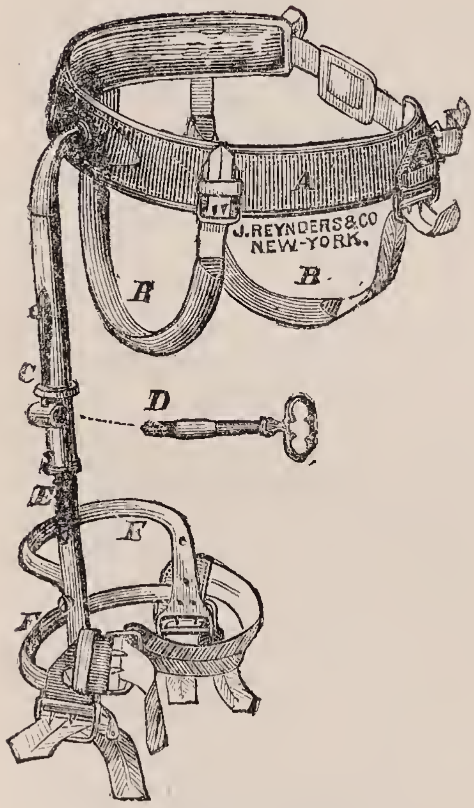


Fig. 1.—Dr. Sayre's short Hip-joint Splint.

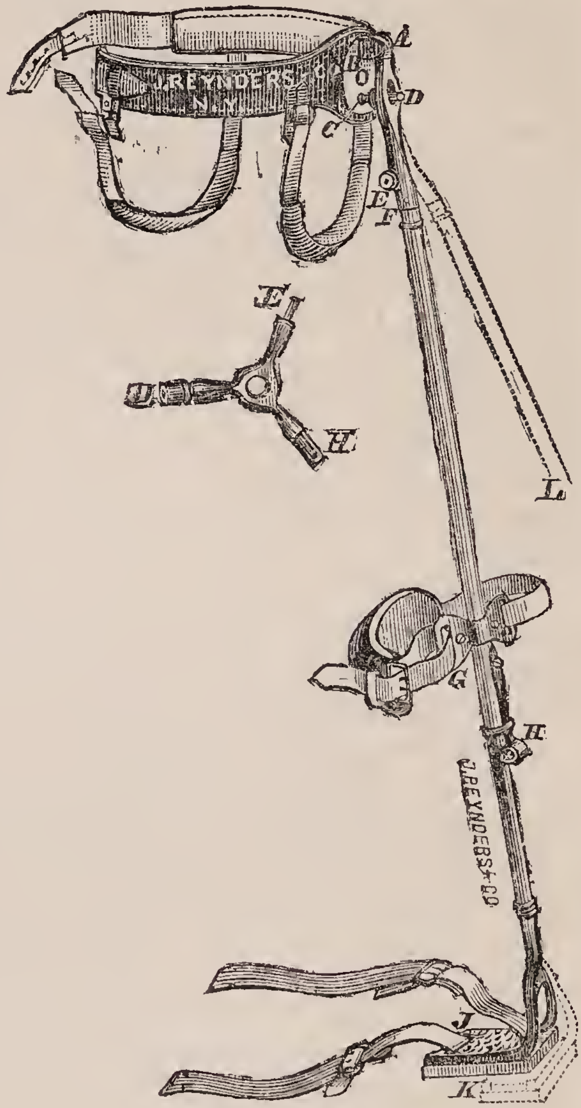


Fig. 2.—Dr. Sayre's long Hip-joint Splint.

My own experience in the use of these instruments is very limited; but during my visit to America last year, I had the opportunity of seeing them applied in a large number of cases, and as it appeared to me with great advantage. In one case, that of a young lady, who was residing in Dr. Taylor's private establishment in New York, where patients are received for the treatment of various deformities, the hip-joint disease appeared to be in a more active stage, judging from the pain she suffered,

than I should have thought the walking instrument could have been applicable; still, when the extending force was applied pretty nearly to its full extent by Dr. Taylor, she was enabled to walk without pain, and therefore it seemed to be a test-case of the value of extension. Children with hip-joint disease in a more chronic form are frequently seen walking about the streets of New York wearing these supports, and are enabled to get in and out of the tramway-cars without difficulty.

Dr. Sayre applied his walking instruments (represented in fig. 2), made by Ernst, of London, a fortnight ago, to one of my cases, a young gentleman six years of age, from Ireland, and he was enabled to walk about with ease and comfort; but as he was previously free from pain, the case was less a test of the value of extension than that previously alluded to.

These instruments and their mode of application are described by Dr. Sayre in his lectures on Orthopædic Surgery, published in 1876; and, as we have now the pleasure of welcoming Dr. Sayre amongst the distinguished guests present at this meeting of our Association, I am glad to state that he will exhibit the hip-joint instrument of his own construction, with the latest improvements, and make some observations on its application.

The accompanying woodcuts, which were kindly forwarded to me by Dr. L. Sayre, of New York, for the purpose of illustrating this paper, represent the two hip-joint instruments used by him for combining extension with motion in the treatment of hip-joint disease. Both these instruments are figured and their application described in Dr. Sayre's recently-published Lectures. —*British Medical Journal, Jan.* 5, 1878, *p.* 10.

37.—ON THE TREATMENT OF COMPOUND FRACTURES.

By Professor Richard Volkmann.

[During the past four years and a half Prof. Volkmann has had under his care seventy-five compound fractures occurring in seventy-three patients.]

A small proportion of these—eight altogether—had to be subjected to secondary amputation. But neither of those who were treated throughout on conservative principles, nor of those where the conservative treatment failed, and where secondary amputation had to be undertaken, did one single patient die; *they one and all recovered!*

And now for the technical part of the antiseptic process. *The first dressing decides the fate of the patient and the course and issue of the wound.* All counter-incisions must now be made and drainage-tubes put in; loose splinters of bone must be removed, and the fractured extremities put into position; any

little sharp projections may be rasped away; and the wound then must be completely disinfected. If this is done thoroughly and with care, the knife need never be taken into the hand again, however long the healing may require; nor will any further drainage-tubes be required, though possibly some small bits of necrosed bone may require removal; but this should only be done when all danger to the patient is past and over. Here is ample ground for doing the first dressing with the very greatest care, and with the most pedantic minuteness. We must not hesitate to bestow half an hour, or even three-quarters of an hour if so much be necessary.

I do not generally undertake this first dressing in the ward itself, but in a special operating theatre. The floor is asphalted, and provided with a drain and waste-pipe, which allow a very free use of water and carbolic acid; we are well provided also with irrigators and douches, disinfected sponges, and a plentiful supply of linen, bandages, and instruments. The patient is generally chloroformed, and then a thorough cleansing of the injured part and of the limb with soap and water, scrubbing-brush, and razor (if needs be), &c., can be conveniently carried out. In all cases the wound is enlarged sufficiently to admit the finger freely, or to allow of the seat of fracture being seen when the soft parts are retracted. The finger having been introduced, is made to cleanse (with a stream of carbolised water) the wound and all recesses opening out of it: every trace of coagulum is to be carefully washed away. If there are any deep pouches they must be incised, and preferably so at the extremity of the *cul-de-sac*, in order to thoroughly drain them. Especially, too, in any places where the skin is separated from its subcutaneous attachments, must incisions be made here and there, as circumstances seem to require, in order to let out any blood, whether liquid or partially coagulated, which may have collected, and drainage-tubes must also be introduced. Any muscular shreds which may have been too severely bruised may be at once removed with scissors.

If the wound is situated so as not to offer a free and easy outlet for the secretions, a counter-opening at some convenient place must be made, and a drainage-tube inserted.

All loose splinters must be carefully removed; such larger ones, however, as are firmly adherent to the periosteum may be left, care being taken not to loosen their periosteal attachments. If there are sharp projecting ends, round them off with the bone-forceps. Be careful that there are no portions of muscles between the broken ends of the bone, as they prevent union; indeed, they are the most frequent cause of protracted consolidation, and of "ununited" fractures (pseudarthroses).

Having got thus far, the wound may once again be well

washed out with carbolised water, and the remaining part of the dressing must be carried out under the carbolic spray.

First of all, the wound itself and its surroundings are covered with a thick handkerchief-like pad of carbolic gauze. I prefer this to the "protective silk" for the first few days; for the fifty to a hundred layers of gauze which are thus lying on the wound readily absorb both any blood and wound secretions which may flow out. Then upon this comes Lister's dressing proper, which I need not here further describe.

I generally change this first dressing on the following day, or on the day but one after at latest, in order to see whether all is going on properly, and whether the drainage-tubes are *in situ* and acting properly. Subsequent dressings are done every second, or third, or fourth day, according to circumstances; so soon as there is no further secretion from the wounds, after freely squeezing the limb, I remove the drainage-tubes. This is generally done about the third or fourth day.

The antiseptic dressing must be continued until the coagula, filling up the wounds, have become organised, or until their place is taken by granulation tissue. Sometimes the long-continued contact of carbolic dressings gives rise to a dermatitis. In such cases the skin may be gently rubbed with some antiseptic oil or ointment—such as boracic ointment—which will protect the skin, and still be antiseptic. In cases where the carbolic treatment disagrees radically, a constant irrigation with a solution of salicylic acid in water, as proposed by Thiersch, may be carried out.

In the foregoing lecture I have endeavoured, gentlemen, to make you acquainted with a method of treating wounds which entirely neutralises the danger of even the severest injuries, and which, in addition to this advantage, is free from pain. It insures cleanliness, does away with foul smells, protects the bed-linen, and paralyses the great evils which the overcrowding of hospitals would otherwise bring along with it. The dressing requires changing but seldom, and thus in large hospitals the work is considerably lessened.

The question of costliness is only an apparent disadvantage, because the shortened residence in hospital which these patients require more than makes up for the first outlay, which, no doubt, is rather large.—*Med. Times and Gazette, Nov.* 24, 1877, *p.* 561.

38.—ABSTRACT OF A LECTURE ON CASES OF TALIPES.

By Richard Davy, Esq. Surgeon to the Westminster Hospital.

During the winter session (1876-77), I have had the opportunity of showing the students typical cases of the four

recognised forms of talipes, viz., varus, valgus, equinus, and calcaneus. The vulgar name for talipes, in any of its varieties, is club-foot; the surgical word is derived from talipedo, I walk on the ankles (talus=ankle, and pes=foot); varus=bow-legged; valgus=knock-kneed; equinus=belonging to horses; calcaneus=belonging to the heel. Confusion constantly arises at the onset between the distortions *varus* and *valgus*. Practically, if you put your knees in the position of "*genu valgum*," knock-kneed, your feet will as a consequence assume the distortion of *talipes valgus*. By attitudinising, you can readily impress the four cardinal points of the club-foot compass on memory. I have illustrated them on the diagram. (Fig. 1.)

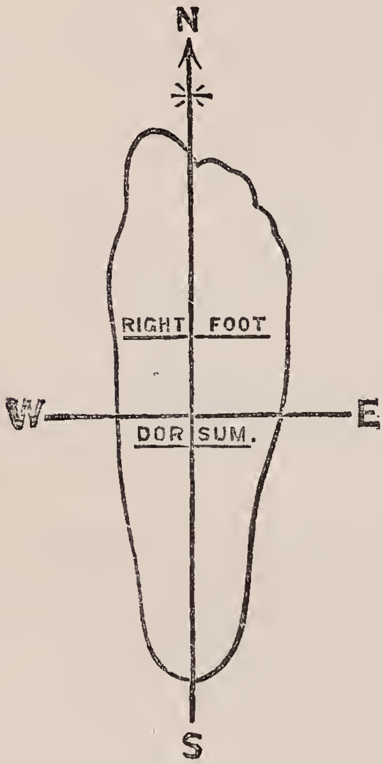


Fig. 1.

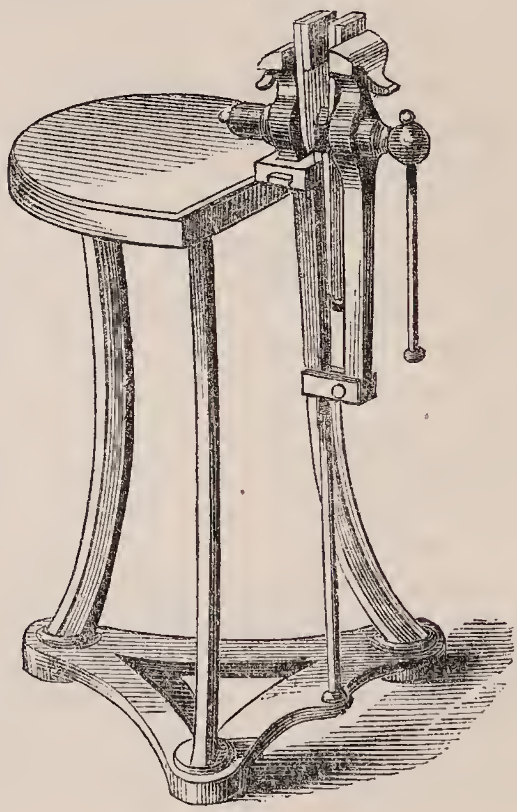


Fig. 2.

Now as we suffer from a north-west or north-east wind, so do we also from talipes equino-varus or valgus: opposite extremes in club-foot do not meet: talipes varus and valgus are antithetical terms, and as far removed the one from the other, as the east is from the west. The utmost aim of orthopædic surgery is to render each case a plantigrade. The casts of each deformity are set out on the operating-table. Club-foot depends pathologically on a variety of causes, *e.g.*, *mechanical*, from the result of burns, removal of bone; *mal-innervation*, both intra-uterine and extra-uterine; *malformation of parts; rachitis, &c.;* but the more I study these inaccuracies, the more I am convinced that the nervous system plays an

important primary *rôle* in their origin and persistence; and that the muscular and tendinous structures are secondary exhibitors. Study, for example, this case.

M. F., aged 8, was admitted under my care on March 26th, 1877, the subject of talipes varus of the left foot. On May 29th, 1876, while he was running away, another boy in spite threw a glass bottle at his leg, which severed his peroneal nerve. The result of this accident was that gradually talipes varus has developed itself; flaccidity over the fibula; and inability to walk. Here is the scar; and we can eliminate the anterior tibial and cutaneous nerves, because the sensibility of the dorsum of his foot is unimpaired.

Let me next show you this boy; an exaggerated case of talipes varus of both feet, and congenital. This case is the third instance in which I have accurately removed a wedge-shaped portion of the tarsal arch for relapsed talipes varus. I have not yet received much support from the profession in performing this operation, Mr. Davies-Colley, of Guy's Hospital, being (so far as I know) the only surgeon who has as yet practised a somewhat similar operation with success. And here let me state, once for all, that I am in no way indebted to any surgeon for the line of practice I have pursued, with the exception of the late Mr. Solly, of St. Thomas's Hospital, who, in my opinion, was right in performing ablation of the cuboid as a step in the right direction for the treatment of confirmed varus.

F. E., aged 12, Clapham, was admitted under my care on November 7th, 1876. On November 14th, 1876, I operated on the right foot, and he was convalescent on December 30th. On January 16th, 1877, I operated on his left foot, and he was convalescent on March 1st, 1877. He shall to-day (May 18th, 1877), walk, hop, run, and jump without any inconvenience and without any mechanical appliance, in the presence of you all. [After these evolutions had been gone through, the boy left the hospital.]

I will now state the line of argument that has led me to advise and practise this operation; and finish up by minutely describing the details of procedure. In a clinical lecture delivered in this theatre, March, 1876, and printed in the British Medical Journal, April 29th, 1876, I gave my reasons for practising ablation of the cuboid, and illustrated practically its results in four cases. My dissatisfaction at the ordinary treatment of talipes varus by division of tendons and manipulation was based upon five years' experience at the Surgical Aid Society; where constantly relapsed cases were brought before my notice, which had been treated by our best orthopedic surgeons.

In January, 1874, I commenced attacking the tarsal arch by excising the cuboid bone; and to-day I am ready to defend not

only ablation of the cuboid, but an accurate removal of a wedge-shaped block of the tarsal arch. I have performed this operation three times in hospital practice with most excellent results; and I will now show you my original instruments, and the method of procedure. In the first place, you must fix most securely the foot on which you operate, and no dresser I have ever yet met with can hold a foot sufficiently steady for the precise use of the chisel. For this reason, I have introduced an ordinary portable vice into our operating theatre, with its jaws defended by the common cork clamps (as used by gunsmiths).

You must prepare yourselves in England to encounter opposition to any new project; and this vice in this theatre was severely criticised before it was finally purchased. I have no hesitation in stating that a good vice for this operation is a *sine quâ non* (Fig. 2). Having put on Esmarch's bandage, accurately fix the leg and ankle in the vice; make an H-shaped incision over the enlarged bursa overlying the cuboid; dissect back double door flaps; and insert stout silver wires to act as

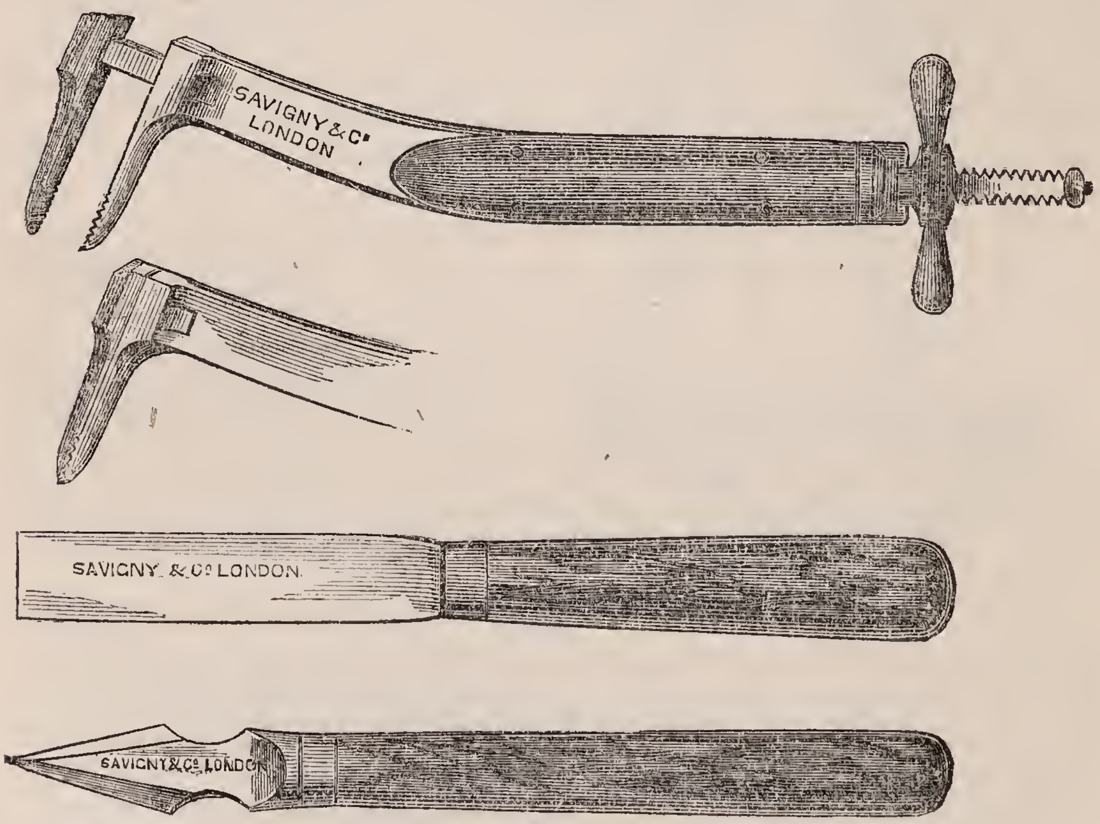


Fig. 3.

retractors: keep close to the bones, above and below, and clear a V-shaped space on the dorsum and sole of the foot, taking for the apex of the triangle the semilunar crease of skin that invariably exists on the inner side of the foot, the stereotyped line at which inversion acts on the soft parts, as it were on a hinge: then use these chisels—painters' knives (Fig. 3)—and

accurately excise the wedge of the tarsus; this will embrace the cuboid, the head of the astragalus, part of the scaphoid, the base of the little metatarsal, and a chip of the external cuneiform bone; use the bone forceps shown above for extracting the wedge. Approximate the gap, and chisel off right and left laminæ of bone until symmetry is restored; rotation of the phalangeal portion of the foot is also now performed, until the foot becomes plantigrade; close the wound by tying the retracting wires together; then fix the foot in this splint (Fig. 4), and put up leg in a gum and chalk bandage over waterproof splintage or flannel roller; swing the foot so that the wound outside is dependent; evert foot-piece until contour of foot is natural. The subsequent bleeding is not alarming; the pain is by no means urgent; swelling results; synovial discharge follows; and, so far as experience demonstrates, the wound is healed and

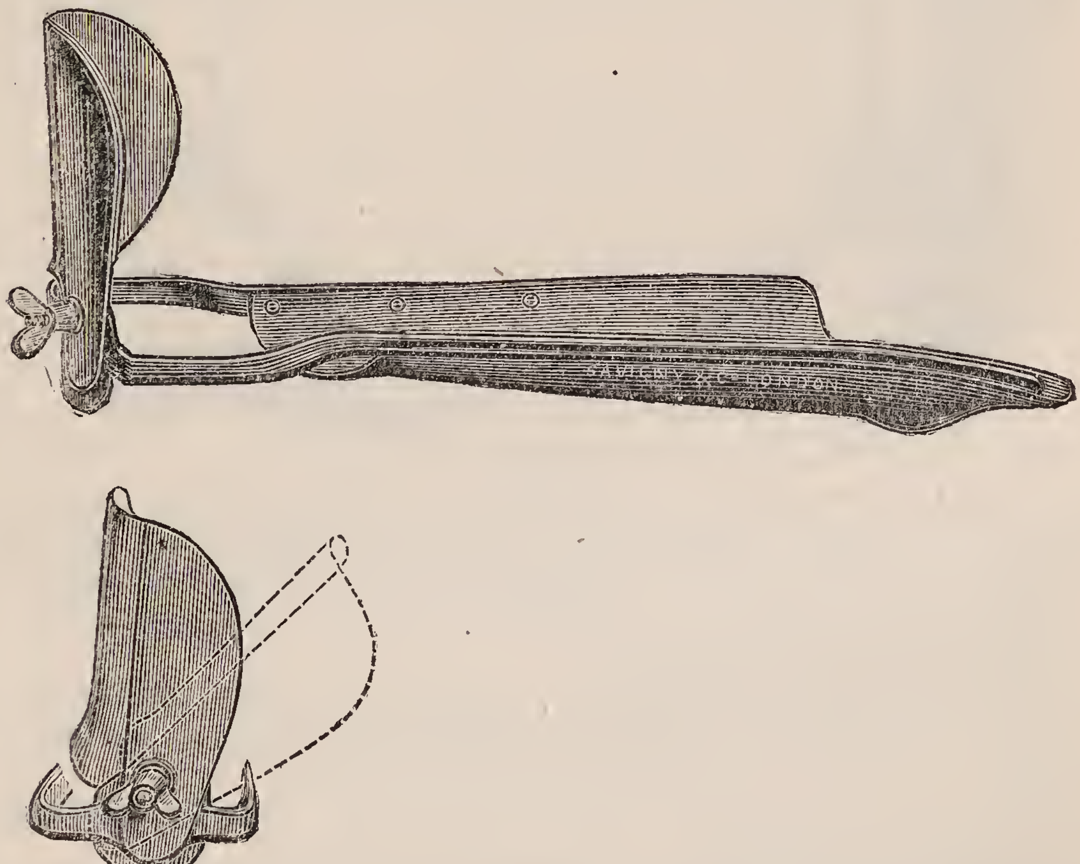


Fig. 4.

the patient convalescent and able to stand in from six weeks to two months.

Now be good enough not to go away with the idea that every case of talipes varus is to be treated in this heroic fashion; this operation is a *dernier ressort* for obstinacy; an ordinary outside splint suffices for babies, with a gum and chalk bandage. In pedestrians, I have used the splint here engraved with admirable results (Fig. 5). The boot with plantar hinge is in this instance

applied to a valgus left foot; the crucial strap of elastic or leather inverts the foot; for varus the steel rod is to the inner

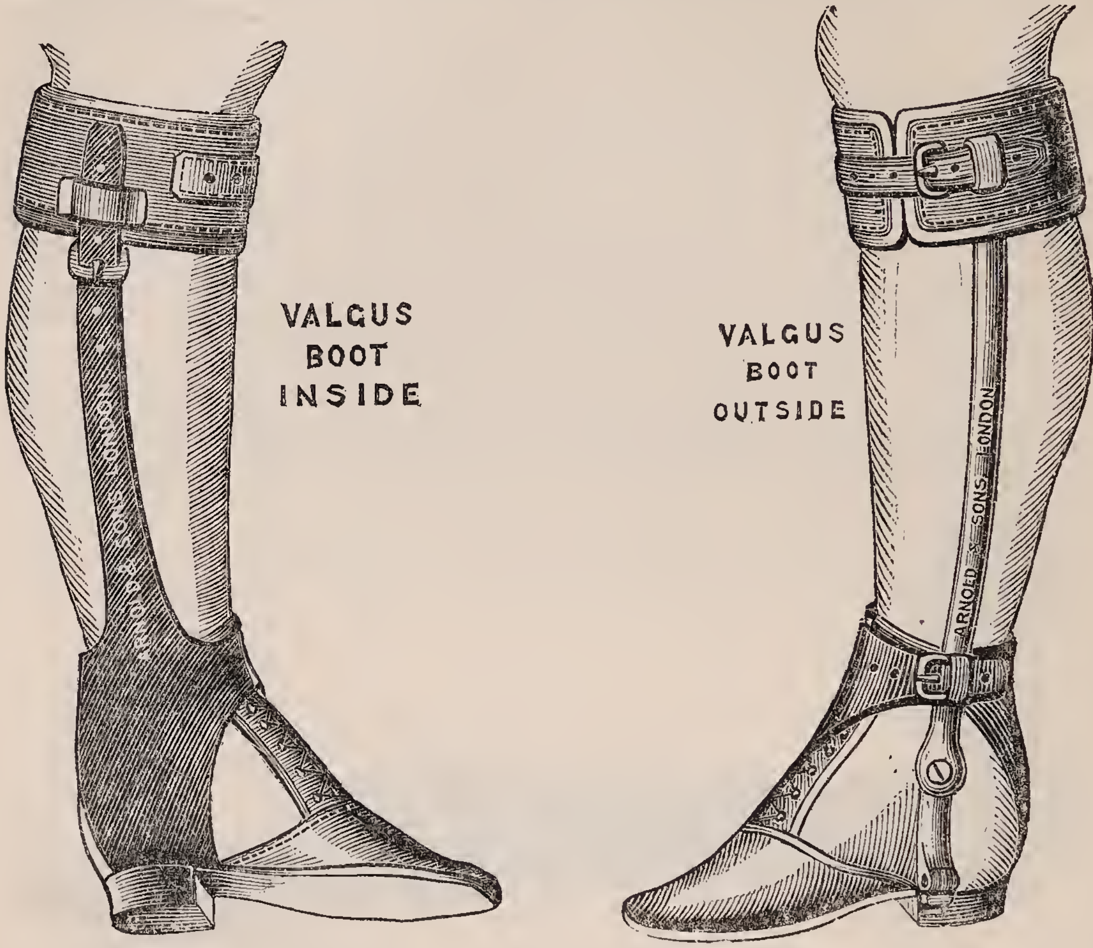


Fig. 5.

side, and the crucial band to the outer; supplying an evertive force. No strap arrangement maintains discipline of natural contour more efficiently. This boot is by no means inelegant, and is made by Messrs. Arnold and Sons. The instruments are supplied by Savigny and Co.

The consideration of tenotomy, and the special treatment for talipes calcaneus and equinus, I will enter upon at a future date.

I will, lastly, read to you an extract from a letter received from the boy's father:

"I beg to take the liberty of offering you, in the name of my wife and myself, sincere thanks for the great benefits my son F. E. has received in the Westminster Hospital. All previous attempts to effect a cure having failed, we are delighted to know that he is now able to run about with ease and comfort."

[Since the delivery of this lecture, I have treated a case of talipes equinus (nineteen years' duration) by an accurate excision of the keystone of the tarsal arch.—*British Medical Journal, Dec.* 15, 1877, *p.* 839.

39.—ON INJURIES OF THE HEAD.

By John Eric Erichsen, Esq., F.R.S., Surgeon-Extraordinary to the Queen; Consulting Surgeon to University College Hospital, &c.

Anybody who has seen an ordinary scalp wound will have noticed how freely it bled. The blood spurts out; the hemorrhage is profuse and difficult to control. What is the reason of this? If you look to the structure of the scalp, you will find two or three points that will explain it. In the first place, the scalp is extremely vascular; it is supplied with many blood-vessels, which anastomose freely through it, and not only is the vascular supply unusually free and abundant, but these vessels are serpentine. You will see on the scalp of old people, when the fatty matter has become absorbed, the serpentine course of the arteries mapped out very plainly. The amount of vascular supply will explain the hemorrhage; but to what are we to refer the frequent uncontrollable character of the hemorrhage? It is to this: these vessels run between the skin and the tendon of the occipito-frontalis; they run in a quantity of very dense granular fat, which lies between the skin and occipito-frontalis. In consequence of lying in this dense granular fat, you will find that these vessels, when divided, do not retract, and they cannot contract; and it is owing to this want of retraction and contraction in consequence of lying in this dense cellulo-adipose tissue that the natural processes for the arrest of hemorrhage do not readily take place in the scalp. This is a very important point in surgical operations, as for the removal of those little sebaceous tumours which are so common on the heads of some people; for if you are not very careful, and go too deep, you will get profuse hemorrhage. In operating upon one of these tumours, then, it is better simply to split its upper part, and not to divide it down to the base; then pull it out by its capsule without dividing it completely through. But if you divide the base and detach the two parts separately, it will be very difficult to get it out clean, and may require a dissection when it would otherwise have been unnecessary to do so. In cases of injuries of the scalp, where the hemorrhage is difficult to arrest, how are we to stop it? We may always stop it at once by acupressure. You can always pass a harelip pin under the bleeding point without any difficulty.

The next point is with regard to the question of erysipelas. It has long been the opinion of surgeons that injuries of the scalp were more subject to erysipelas than wounds of other parts of the body. That is to say, that the accidental circumstances which occasion wounds of the scalp favour erysipelas; but that in the anatomical relations there is no reason why it

should be more frequent in the scalp than in other parts of the body. There is, however, one great anatomical peculiarity that leads to the more frequent occurrence of erysipelas here than elsewhere—namely, that the occipito-frontalis tendon lies upon a plane of very loose and lax areolar tissue, which intervenes between it and the pericranium; and if that layer of areolar tissue be laid open, you are very apt indeed to get profuse suppuration there, and you get erysipelas of the scalp—you get a kind of cellulitis, which renders wounds of the scalp that penetrate deeply very dangerous. There is, in point of fact, this difference between erysipelas of the scalp affecting the skin only—the idiopathic,—and that implicating the occipito-frontalis between it and the pericranium—the traumatic. In ordinary idiopathic erysipelas of the scalp death scarcely ever occurs. I think I can scarcely recollect ever having seen a person die from erysipelas of the scalp that was not traumatic. Death of course may occur, but it is extremely rare. It is very different with traumatic erysipelas of the scalp, which is extremely dangerous and very often fatal.

In traumatic erysipelas of the scalp you have erysipelas of the large plane of areolar tissue between the occipito-frontalis and the bone, which is very apt to implicate the brain and its membranes, The erysipelas is not dangerous as far as the skin is concerned, but where this large cellular plane is opened, which is especially liable to happen in wounds. Most wounds of the scalp are somewhat unfortunately situated, so far as the healing process is concerned, When a person is struck upon the head, the blow has a tendency to pass down, leaving a kind of bag downwards with the open end upwards. Hence there is always a tendency to the accumulation of extravasated blood, sometimes to retention of foreign bodies, and eventually pus in that pouch or bag which is the most dependent part of the wound; and it is this that causes the frequent occurrence of erysipelas of the scalp. In order to prevent this, you must make counter-openings, and a drainage-tube may be introduced in order to facilitate the escape of the pus. By making these openings in the early state, you will find the tendency to cellulitis and erysipelas of the scalp is very materially lessened. It has been suggested that the application of sutures tends to produce erysipelas, and it is so; but it is not the mere introduction of the stitch, for it is absurd to think that the introduction of a few stitches into the edges of a great laceration could increase materially, if at all, the tendency to erysipelas, and they do not do so. I believe there is no more danger in passing a stitch through the scalp than in passing a stitch through any other part of the body. The mere introduction of sutures through the scalp does not occasion erysipelas. You may pass

sutures round a nævus, and you never get erysipelas. But they do cause erysipelas indirectly, for by stitching up the scalp you stitch up the mouth of the bag, and prevent the drainage and escape of blood from it. Stitches should not be used in these cases unless there is a free drainage, and then there can be no objection whatever to a few stitches being introduced to keep the lips of the wound together. It depends entirely upon how you treat the bag itself: if you close the mouth, you do an infinity of harm; if you open the pouch as well from the bottom and drain it, then the stitches do good rather then harm. It is usual in these cases to bandage the whole of the head, and you will see in some books on surgery very complicated and artistic methods depicted to cover the head with a bandage like a nightcap. These things are not only useless—they are worse than useless, for they are dangerous. You cannot compress the scalp accurately against the skull in the way that these are directed to be applied. They merely keep up pressure, and are quite useless so far as support is concerned. Besides this they are injurious by heating the scalp and preventing the free drainage of pus, &c. I should advise you to see that the wound is well drained, and when it is thoroughly well drained, it may be brought well together above the drainage-hole.

There is a condition which is very apt to follow when this areolar plane is opened, and bagging of inflammatory products is allowed to take place, and which is very apt to be mistaken for erysipelas, and that is abscess. The diagnosis is not easy. In both cases you will have the eyelids puffed, and a puffy swelling of the scalp at the same time; but there are two or three points, attention to which will enable the diagnosis to be readily made. The first point is that in erysipelas you always get the ears involved; they become red, swollen, and covered with blebs. In abscess this does not happen. In abscess the presence of fluctuation is always determined by the attachments of the occipito-frontalis muscle. The swelling presents itself at three points—namely (1) posteriorly, at the attachment to the superior curved line of the occipital bone, and the pus will gravitate down and form a bag there, but it will not go any lower in the neck; (2) if it comes to the side of the scalp, it will present above the mastoid process, and on a level with the zygoma, but it will not descend below the zygoma; and (3) if it comes to the front, in consequence of the fibres of the occipito-frontalis being blended with those of the corrugator supercilii and the orbicularis palpebrarum—it will have a tendency to gravitate into the the eyelids and form a bag, and it is in one of these situations that these bags of pus will develop themselves, lying between the occipito-frontalis and skull.

Well, now, there is this third point of special interest in injuries of the scalp, and that is the reparative power that is evinced by the scalp, which is very remarkable. The scalp stands in quite a different category to other portions of the integument. You may remove portions of the scalp, and cicatrisation takes place very rapidly indeed. This will explain the good results that follow when a nose is made from the scalp by the Indian operation. Although a large portion of the scalp is turned over from the forehead, very little scar is left. In this operation we have a good illustration of the reparative power possessed by the scalp. I have seen, in a person who had fallen into the fire in an epileptic fit, half the scalp removed from the side of the head, and a portion of bone detached by a process of exfoliation. But the wound contracted, and left a firm cicatrix. Some years ago I removed a very large epithelioma, considerably larger than the palm of the hand, from the side of the head of an elderly lady, a patient of Mr. Lunn, of Hull. I removed it with the galvanic écraseur. A portion of the bone that was touched with the hot wire exfoliated, but the wound, which was very extensive, cicatrised well.

In injuries of the scalp it very commonly indeed happens that the brain is more or less injured, for it is seldom that a person gets a blow upon the head so as to lacerate or seriously contuse the scalp without the brain being injured at the same time. The patient in this case is stunned: to use the popular expression, "knocked silly;" to use the surgical expression, suffering from concussion. I am going to mention one or two points in practical connexion with this condition. In the first place, if a person has been stunned, it is a very important thing in his after history. The total annihilation of voluntary motion and of sensation indicates that a deep impression has been made on the nervous system. We know, for instance, what happens to a person who has had an epileptic seizure, however slight it may be. He may lose consciousness only for a minute, and have a slight twitch somewhere in the body, but yet the whole of that man's life is completely changed; he becomes a different being, socially, physically, and morally. It is the same if a person has been stunned, and it is very difficult to say what may happen after that occurrence. Stunning, with complete loss of consciousness, is a very serious matter indeed. It indicates commotion of the brain-substance, and great evils may follow. It is most important, therefore, to recognise *true* stunning in contra-distinction to *false* stunning. Let me give an illustration. A person is in a carriage accident: the horses run away; he is thrown off the box, and pitches upon his head. He is picked up in an unconscious state. The man is

stunned, there is no question about it. When he recovers you ask him what he remembers about the accident, but he does not remember anything about it. There is a complete gap in his memory; he forgets everything up to a minute or two before the accident, and there is often no recollection of the very circumstance that preceded the accident, and this is a very remarkable psychological condition. He forgets some of the events *immediately antecedent* to his becoming unconscious. This is a most remarkable mental phenomenon. The patient does not only forget the circumstances of the accident, but also those that immediately preceded it. The horses ran away, and the man is pitched upon his head, and the last thing he recollects is that a pig, or something of that kind, ran across the road, and frightened his horses. He does not recollect what took place between the time when the horses took fright and the time that he received the injury which rendered him unconscious. But a lady is in the carriage, and she is thrown out. She is also, after a time, picked up in a state of utter prostration, not injured about the head, and with no serious injury about the body. She falls back into the arms of those who pick her up, and is carried into a neighbouring house in a state of unconsciousness. Here restoratives are applied, and after a time she recovers herself. The two people look alike, but in reality the condition is quite different. The woman not only recollects everything that has occurred throughout the whole of the period until after the danger is over, but the minutest circumstances are strongly, and remain deeply, impressed on her mind; and then, when all danger has past, she loses her consciousness; she faints, in fact. That is not a condition of concussion, or of true stunning. The two cases are quite different, not only in their immediate characters, but will probably be so in their remoter results, and should not be confounded. The psychological phenomena that follow the recovery from concussion are very curious, and they are very important. There was a man in Ward 1 some years ago who had a head injury. He would give the story of his accident in the most distinct, intelligible, and circumstantial manner, but he would vary that story according to the wish and suggestions of the inquirer, and he would tell half a dozen different stories in a very short time, and so he would give any series of stories, and all in the most circumstantial manner. This is very curious, and it is very important in a medico-legal view, because if this man had been brought up before a magistrate he might have said that somebody had struck him on the head, and he would easily have been led to give a most circumstantial detail of an assault that never occurred, and that would have been a pure fiction. It is a very curious mental study to trace the phenomena that sometimes follow these injuries of the head.

There is another condition to which I wish briefly to direct your attention, and it is not so much for concussion that it may be mistaken, but it is a condition which is liable to be confounded with compression. It sometimes happens that a person is picked up in the street with an injury of the head, and the question arises—Is this man drunk, or is he insensible from the injury that he has sustained?—in fact, the question which agitated the public mind some time ago: "Is he drunk or dying?" But it would be more correct sometimes to say that he is both—that he is drunk *and* dying too. It is very important indeed to determine when a man is simply drunk, although he may be "dead-drunk." It is usually not very difficult to make the diagnosis. His friends know what has occurred to him, and he smells strongly of drink, and is in that helpless condition which you see if a person is suffering from alcoholic poisoning; but if associated with this state there is an injury—in going out of the public-house he reels and falls down, and is picked up with a great cut in his head,—then the question arises: Is this man drunk, or is he suffering from head injury, or a combination of the two? The difficulty in answering this question is extremely great—in fact, I believe it is often impossible; and the only method of giving an answer to this question is by waiting and seeing the result of what happens. You will find in this as well as in other cases of surgery that time is the best diagnostician after all—that what is most difficult at the moment will become clear if you allow a few hours, days, or weeks to elapse. If you are called to a person with a head injury, who smells of drink, and has been undoubtedly drinking, and is more or less suffering from alcoholic poisoning, it will be the wisest plan to examine him most carefully to see if there is any injury. If there is, the question of diagnosis must be reserved. He may be relieved of his alcohol by means of the stomach-pump, &c., and then wait and see what happens. It not uncommonly happens that people are brought to the hospital in this state, and are sent away as only drunk. They may be drunk, but at the same time they may be suffering from a very serious injury. Some years ago I was called to a gentleman who had been brought to a hospital in a state of semi-insensibility, with a bruise on the forehead. He had evidently been "dining out," or something of that kind, and was looked upon as being drunk, and was sent home in that condition. My friend who had been called in asked me to see him, and we diagnosed that he had a fracture of the skull. He died, and the post-mortem confirmed our diagnosis, for there was an extensive fracture across the base of the skull. These cases are very apt indeed to happen in ordinary hospital practice, and there is

not one absolute method of diagnosis. I will not go into questions as to the state of the pupils, or of an epileptiform state, which in a person suffering from drink I believe to be all futile. It is quite impossible to effect an immediate and at the same time an accurate diagnosis in these cases, and the only thing to be done is this—to try to relieve him of the alcohol by the stomach-pump, and to wait; let him lie by, in order to see what the result is. For this purpose there ought to be a separate ward, and I have urged it upon the authorities of this hospital repeatedly, so that the surgeon may wait, and see the result. The house-surgeon is called up in the early morning, not perhaps in the happiest state of mind, and the man is brought in between two policemen, reeling, and smelling of drink. The house-surgeon is very apt, though in doing so he is greatly to blame, to take the opinion suggested to him, and send him away as drunk and incapable; or he may make a very careful examination, but how can he tell that that man has not a lacerated liver, or a ruptured meningeal artery, when he is possibly drunk as well as injured? Such a patient as that should be taken into the hospital, but he could not be taken into the general wards, for that would be very unfair to the other patients, and might be very dangerous to them by the disturbance he would create. If there was a small ward to put him in, then there would be no eventual difficulty in the diagnosis; and the patient would be placed in a condition of safety such as humanity would suggest for him. Such a ward is imperatively necessary in all hospitals for two reasons: first, for the safety of the patient; and, secondly, to free the house-surgeon of the responsibility of these cases, because it is impossible for any physician or surgeon to discover off-hand and say what the exact cause of the symptoms of coma or compression of the brain may be. This diagnostic point cannot be established by any practitioner, however skilled and however experienced, unless you give him time. With regard to the actual condition of the brain in many of these cases of extreme concussion, it is difficult to say what is the state of things. In those cases that have died, small extravasations of blood have been found; but in those cases that have recovered, it is very uncertain what condition the brain is in. You must recollect, gentlemen, that what you see in the dead-house is a condition of things no longer compatible with life; for you never see in the dead-house a condition of things such as exists in the living body, and that can be recovered from. There is very little known of the actual pathology of concussion of the brain. You may get total annihilation of mental power without any physical lesion or morbid condition of any kind; just as you annihilate the power of a magnet by giving it a sharp blow with a hammer.

So in many cases of concussion it is doubtful whether any visible effect is produced on the substance of the brain.—*Lancet, Jan.* 26, 1878, *p.* 115.

40.—ON THE TREATMENT OF WOUNDS.

Mr. Philip Cowen has recently written a paper on the simple methods of treating wounds, in which he states that, armed with some thick, well-made tincture of benzoin, ligatures, iron wire sutures, compresses of lint, cotton wool, bandages, simple ointment, and water, the practitioner can most successfully treat all wounds that may come under his notice, rapidly and with the almost certain hope that most of them will unite by adhesion. The treatment of a fresh wound is essentially different from that of a non-recent wound, *i.e.*, those that have been inflicted twenty-four hours and upwards. A fresh wound may be sealed up, whereas in wounds after a few hours, certain decomposing products, however slight, are found, viz., ill-smelling serum, decomposing blood, pus, &c., from within, or caused by germs from without, which render sealing-up an absurd and dangerous procedure, resulting in one or other form of blood-poisoning. Thus there is at once a natural distinction of great importance in treatment—seal up fresh wounds, ventilate other wounds. The most simple application for sealing up wounds is the old-fashioned tincture of benzoin, and it is the most successful. By it nearly all fresh wounds heal rapidly, while they do not do so under watery and fatty dressings. Tincture of benzoin has a remarkable property of uniting tissues and combining with blood. It is antiseptic, and, assisted by cotton-wool pads of lint and firm bandaging, will arrest hemorrhage from all vessels less in size than the radial artery. Non-recent wounds which suppurate it is not desirable to heal by adhesion. The most important item in the treatment of these is ventilation with as pure air as possible. None but the most evil results follow the application of waterproof materials such as oiled silk and gutta percha tissue over the dressings. Such wounds invariably stink and slough; the wound is made unduly hot, products of decomposition are retained, the surface has a greyish grumous aspect and loses substance daily. A simple piece of lint or muslin covered by cerate, or dipped in lotions of Condy's fluid (1 to 40), or tincture of myrrh and water (1 to 20), spirit and water or weak carbolic acid lotion (1 to 60), with just a layer of bandage to retain the dressing in its place, is all that is necessary, save a daily syringing and washing with warm Condy's fluid and water. Soothe, cool, ventilate; if it be desirable to keep the wound moist let it be wetted as often as necessary, avoiding the use of waterpoof coverings. Should

erysipelas, swelling, pyæmia, blood-poisoning, or symptoms of thrombosis arise, freely foment and apply poultices as hot as can be borne. Mineral acids or tincture of iron may be given in free doses internally. Cleanliness in the vessels used for soaking wounds is very necessary, and rag which can be burnt directly should be used instead of sponge. The simplest dressing of all to wounds—leaving them alone—is at times one of the most useful. A large suppurating surface after a burn with flabby and juicy granulation will dry up, its surface become glazed, and rapid healing result, by leaving it exposed to the air for a few hours daily. (Medical Examiner, No. 20, 1877.)—*Practitioner, Feb.* 1877, *p.* 129.

ORGANS OF CIRCULATION.

41.—RAPID CURE OF A CASE OF ANEURISM OF THE ANTERIOR TIBIAL BY ESMARCH'S BANDAGE.

Under the care of Mr. CORNISH, at the Taunton and Somerset Hospital.

For the notes of this interesting case we are indebted to Mr. G. W. Rigden, house-surgeon.

A young agricultural labourer, aged twenty, was admitted into the hospital with the following history :—During the last week of August he wounded his right leg with a scythe. He lost a large quantity of blood at the time, but the wound healed after he had been in bed about a month. When he began to get about he noticed that his foot dropped on that side, and for this he came to the hospital for advice.

On admission, it was found that he could not raise his foot on the affected side, but there was no stiffness of the joint, the whole foot being perfectly flaccid. The cicatrix of the wound was noticed, about the middle of the outer side of the leg ; and beneath this was found an ill-defined tumour deep in the muscles of the leg, which exhibited a distinct pulsation synchronous with each beat of the heart, and on listening with a stethoscope a distinct bruit could be heard. After he had been kept at rest in bed a few days, the tumour became much more defined ; it was less in size, but the margin of it much more distinct ; it was very deep, and appeared about the size of a small hen's egg. There could be no doubt it was a traumatic aneurism of the anterior tibial. It was resolved to attempt to cure it by means of Esmarch's bandage in the manner recommended by Mr. Thomas Smith in the Lancet of May 26th, 1877.

On December 2nd, at 11.20 a.m., a flannel bandage was applied from the toes to the tumour, and a second bandage

from the tumour to the middle of the thigh, leaving the tumour itself exposed. Esmarch's bandage was then applied with moderate tightness from the toes to the tumour, and the patient made to stand out of bed, in order to fill the tumour well with blood. Esmarch's bandage was then applied from the tumour to the middle of the thigh, and the thick india-rubber tubing firmly fixed above it. The tumour itself being still exposed, it was noticed that the pulsation in it was quite arrested, and no bruit could be heard with the stethoscope. The patient was then directed to keep quiet in bed with his leg well raised on pillows. He did not complain of any pain till twelve o'clock (forty minutes), when he began to have the sensation of pins and needles in his foot; this pain had become so intolerable at 12.20 (one hour after the application of the bandage) that a horseshoe tourniquet was fixed firmly at the groin, and the india-rubber tubing and Esmarch's bandage removed, the flannel bandages being allowed to remain. It was noticed that though the colour returned to the limb, no pulsation could be felt either in the tumour or in the femoral artery. A dose of chloral hydrate was given, and the patient directed to keep quiet. At 3.30 p.m. a pad of lint was fixed by strapping on the line of the femoral, and the tourniquet slightly relaxed, It was further relaxed at 4.30 p.m., and removed altogether at 7 p.m. The patient was put on a milk and beef-tea diet, and directed not to move if he could possibly help it.

There has never been the slightest return either of impulse or bruit; the tumour has gradually become smaller till now it cannot be felt at all; the power of lifting the foot returned as the tumour diminished in size, and now, in less than three weeks, is almost natural. The patient will be discharged in a few days.—*Lancet, Feb.* 16, 1878, *p.* 235.

42.—ON THE SPONTANEOUS ARREST OF BLEEDING FROM DIVIDED VESSELS.

By T. Wharton Jones, Esq., F.R.S., Professor of Ophthalmic Medicine and Surgery in University College, London.

Observations and experiments show that from small arteries, when divided, no blood at all may escape, and that this is, in the first instance, owing to the contraction of the muscular walls of the vessel excited by the irritation of the injury, whereby its canal is closed to some extent, both above and below the wound. And though soon after this both segments of the artery become dilated again, their retracted mouths continue constricted, and the surrounding tissue of their sheath is so much closed in around them that blood is prevented from escaping, while collateral branches having become dilated, the

stream of blood—direct in the upper segment, retrograde in the lower—is at the same time diverted away from the mouths of the divided trunk.

When a larger artery is cut across, the retraction of its ends within its sheath and the constriction of its calibre which ensue may not be sufficiently great to oppose the escape of blood. After a few minutes, however, the blood which has escaped coagulates in the wound, and the clot thus formed helps to stop fully up the retracted and contracted mouths of the vessel, and so hemorrhage is arrested.

Leaving the suppression of hemorrhage from the smaller arteries, when divided, and turning to the suppression of that from such as are of a large size, we come to a department of the subject which, from its very great and immediate practical importance, fixed the attention of surgeons at an early period.

Whilst some supposed that hemorrhage was arrested by a coagulum of blood stopping up the mouth of the divided and bleeding artery, others maintained that the contraction of the divided artery itself is the means employed by nature in the suppression of hemorrhage. A third opinion, in which the truth seems to lie, stands intermediate between, and combines these two partial and extreme views. It was thus enunciated by Sharp: "The bloodvessels, after division, at length contracting and withdrawing themselves into the wound, their extremities are shut up by the coagulated blood."

Hewson's opinion corresponded with that of Sharp, and is expressed with nearly equal terseness.

The exposition of the subject, however, founded on a connected series of investigations, was first accomplished by Dr. J. F. D. Jones in his work "On the processes of Nature in suppressing Hemorrhage from divided and punctured Arteries," published seventy years ago.

The action and even the structure of arteries, their sheath and the cellular substance connecting them with it, are, Dr. Jones pointed out, all concerned in the process of suppressing hemorrhage.

"An impetuous flow of blood," said Dr. Jones, "a sudden and forcible retraction of the artery within its sheath, and a slight contraction of its extremity, are the immediate and almost simultaneous effects of its division. The natural impulse, however, with which the blood is driven on, in some measure, counteracts the retraction, and resists the contraction of the artery. The blood is effused into the cellular substance between the artery and its sheath, and passing through that canal of the sheath which had been formed by the retraction of the artery, flows freely externally, or is extravasated into the surrounding cellular membrane, in proportion to the

open or confined state of the external wound. The retracting artery leaves the internal surface of the sheath uneven by lacerating or stretching the cellular fibres that connected them. These fibres entangle the blood as it flows, and thus the foundation is laid for the formation of a coagulum at the mouth of the artery, and which appears to be completed by the blood, as it passes through this canal of the sheath, gradually adhering and coagulating around its internal surface, till it completely fills it up from the circumference to the centre." "A coagulum then," continues Jones, "formed at the mouth of the artery, and within its sheath, and which he (Jones) distinguished in the experiments by the name of the *external* coagulum, presents the first complete barrier to the effusion of blood."

The retraction of the ends of a divided artery within its sheath here mentioned depends on the action of the longitudinal elastic fibres which form the inner layer of the outer coat of the vessel, while the contraction of the mouth of the artery is due to the action of the circular muscular fibres composing the middle coat. In consequence, however, of the proportionally less developed muscularity of the middle coat of large arteries than of that of small, the contraction is correspondingly less in large arteries. Hence the formation of an external clot plays a more important part in the suppression of hemorrhage from large arteries when cut and left to themselves.

This difference between the larger and smaller arteries, in addition to their actual difference of width, is the cause why in the larger vessels a clot of a size sufficient to stop up the gaping mouth of the vessel takes a longer time to be formed, and why, consequently, the bleeding may not stop of itself before it has gone on to a dangerous extent.

The reason why bleeding from small arteries, when divided, more speedily ceases spontaneously than that from larger arteries, is that the heart's action operates less powerfully on the column of blood within them, and that the small arteries, as we have seen, having more contractile coats, become proportionally more constricted when cut across. The formation of a clot, therefore, of a size sufficient to stop the small orifice of the cut end of the artery, and arrest the hemorrhage—all things being equal in respect to the state of the blood generally—more speedily takes place than in the case of a large artery.

While, therefore, hemorrhage from small arteries may safely be left to subside of itself, that from large arteries requires the immediate application of the ligature.—*Lancet, March* 9, 1878, *p*. 342.

43.—TENDON LIGATURES.

At a recent meeting of the Clinical Society of London, Mr. CALLENDER, the President, exhibited some specimens of tendon ligatures, which had been obtained from Dr. Girdlestone, of Melbourne, through the kindness of Mr. Hulme, of Guildford. They were from the tail of the kangaroo, and were preferable to the catgut ligature, in their angular form, allowing of their being tied securely, the knots not slipping, and their solution does not take place so rapidly as the catgut. In Melbourne they were much used as sutures, but, as the supply of kangaroo tails was limited, Mr. Callender had proposed to obtain ligatures from the tendons of the tails of horses. He was having some ligatures prepared, and would state his experience of them at a future time.

Mr. W. D. SPANTON, of Hanley, Staffordshire, writes:—"In connexion with the remarks of Mr. Callender at the Clinical Society on the subject of tendon ligatures, it may be of some interest to your readers to know that Mr. Garner more than a year ago advocated the use of them in a communication he made to the British Medical Association at one of the Staffordshire Branch meetings. Those used by Mr. Garner were prepared from the tendons of the ox, roughly made by simply splitting up the tendon with a penknife, and soaking in carbolic acid. Prepared in this way they are somewhat open to objection on account of their want of evenness; but this might be overcome by greater care, and perhaps by rolling them under pressure. They possess certain advantages over catgut, and deserve a more extended trial. But I cannot help thinking they will prove inferior to silk, and, in certain cases, to torsion as a means of arresting hemorrhage.

"The advantages of tendon are, great strength, a secure knot, and early solution. As far as we have been able to ascertain at the North Staffordshire Infirmary, tendon and catgut of equal thickness take about the same time to dissolve. The following consecutive cases, in which I first made use of tendon ligatures (for a supply of which I am indebted to my friend Mr. Garner), will fairly illustrate their utility.

"1. A girl, aged twelve, whose knee I excised for malformation, resulting from old synovitis. In this case five tendon ligatures were employed, being cut off short. The bones were firmly united, and the wound quite healed at the end of three weeks. There was no hemorrhage. Nothing was seen of the ligatures.

"2. A woman, aged forty, whose thigh was amputated in the lower third for primary encephaloid cancer of the head of the tibia. Four ligatures were employed, one being used for the

femoral artery. No hemorrhage occurred. Nothing was seen of the ligatures. Suppuration was rather profuse, but the case did well.

"3. Excision of the elbow. Four ligatures were used in the same way, and with a similar result.

"These cases will suffice to prove that the tendon ligature is trustworthy.

"If tendons from the horse are employed, care will have to be taken that the animal from which they are procured is perfectly healthy, or the results of the use of tendon ligature may lead to its untimely disuse."—*Lancet, Feb.* 16, *and March* 9, 1878, *pp.* 239, 370.

ALIMENTARY CANAL.

44.—ON A CASE OF NASO-PHARYNGEAL POLYPUS.

By William S. Savory, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

The form of tumour called polypus is more common in the region of the nares than in any other part: here it varies in structure and site. While from their shape they all are properly termed polypi, some—and these are the most frequent—have a simple loose fibro-cellular structure, with much fluid in the meshes—the familiar mucous, gelatinous, or, as it is now termed, myxomatous polypus; some are firmly fibrous—fibromata; while others, consisting of the more immature elements of connective tissue, are sarcomata. I need not add that these several kinds of polypi vary in their nature: some are innocent, others are recurrent, or even malignant. Let us, however, be clear about this. Every one knows that even the simplest and most innocent—the common polypus of the nose—very frequently recurs after removal. It is apt to grow again and again, and its complete and final extirpation is at times a matter of very great difficulty; while even the sarcomatous polypus—the kind consisting chiefly of immature forms, the texture which emphatically suggests recurrence—may yet, provided it can be entirely removed, even to the base of its stalk, and the surface from which it grew destroyed, never return. Indeed, in these polypi, beyond all other tumours, this seems to be a point of the first importance. When, after an apparently satisfactory removal, the nostrils again become plugged with the ordinary gelatinous polypus, either (which is only too likely) some fragment of the original stalk has been left, or other small polypi, which, having been compressed by the first, could not expand, as soon as they obtain space rapidly fill up the nostril. Pott, in his chapter on Nasal Polypus, very clearly points this out (see works by Earle,

vol. iii.). I fancy these common polypi are more often multiple than they are usually supposed to be, because after the entire removal of a large one with a typical shape and perfect stalk, I have often seen the apparently vacant nostril rapidly occupied by several very small and distinct growths; and for this reason it is always well to employ an astringent for some time after an operation. On the other hand, we see cases in which when a polypus possessing a structure suggestive of very ugly inferences is cleanly and thoroughly swept away, it never recurs. Of course, there is a wide difference in the liability to recurrence of these various kinds, determined by their nature and independent of the mode of removal; and perhaps, a yet more important distinction, in the fact, that while the more innocent forms, although recurring for many years, are not wont to trespass beyond their original site, the worse forms are sadly prone to extend their base of growth, and to invade and occupy without scruple adjacent parts. They may grow from any portion of the surface, either of the nares, sinuses, or pharynx. They often, in their progress, expand, and sometimes even pass through bone and intrude on the brain (see Preparation 2,210*a*—fourth volume of Pathology—in the museum of the Royal College of Surgeons).

Another fact which, in more than one way, is of great clinical importance, is that these several kinds of polypi are apt to spring from different parts, so that the situation of a polypus, or more precisely of the surface from which it grows, is strongly suggestive of its structure and character. Every one, for instance, knows how the common gelatinous polypus affects the turbinated bones, and how usefully, when we proceed to remove them, we are guided by this knowledge; whereas the polypi which occupy the pharynx are fibrous or sarcomatous, these latter springing far more indifferently from any portion of the surface of the naso-pharyngeal region. Their favourite sites are the posterior border of the nares, about the edges of the pterygoid plates, sometimes the margin of the septum or the upper wall of the pharynx, the under surface of the body of the sphenoid or basilar portion of the occipital.

Some one may perhaps ask why I have not mentioned cancer among the forms of polypi. Because I do not think that cancer, in its proper sense, forms the structure of polypi, strictly so called, whether nasal, pharyngeal, or naso-pharyngeal, to which form of tumours these remarks apply. You meet, of course, with cancer in the nose and pharynx. It will grow into and destroy those parts, but not as a polypus; not, I think, as a pyriform tumour with a well defined stalk, springing either from the mucous membrane or the fibrous tissue between it and the bone. The stalk of the firmer kinds of polypus is usually, indeed, directly continuous with the periosteum.

Now, I must not here enter at any length into the question of treatment of these various kinds of polypi. The principle is the same in all: to remove as cleanly as possible the whole of the growth, to detach thoroughly the base of its stalk—to destroy its roots, as some say. In order to secure its thorough extirpation, we do not think of attacking the bulk of the tumour; its neck and base are the parts at which we aim. We may grasp the pedicle with strong forceps, or secure it by a ligature, or use the knife or *écraseur*, or cautery, or carry out the principle in other ways: but the operation is never satisfactory when the polypus is brought away piecemeal or torn and mangled. The simple object is, I repeat, to bring the whole thing away at once by its neck, and to leave a bare surface, with no fragment projecting beyond the level of the surrounding mucous membrane. In the case of the more doubtful forms, it is well to cauterise the surface afterwards.

In accomplishing this, lies the whole art of removing these polypi; and I need not tell you that the degree of difficulty of this operation varies widely in different cases. In the nose, we are guided by our knowledge of the parts apt to be involved, sometimes assisted by the gentle and cautious use of a probe, with which we may define the neck. When they occupy the upper part of the pharynx, we examine with our finger passed up behind the soft palate, assisted usually by a director or something like it, passed through the nostril. But from whatever part the tumour may grow, the difficulty of defining and commanding it is, I think, always invariably increased by any previous attempt at extirpation. When these polypi have been rudely attacked and injured, their usual form is altered; the distinction between the base of the tumour and adjacent parts becomes obscured. You cannot then so clearly distinguish between what ought and what ought not to be there. It is certainly most satisfactory to have to do with polypi which have not been previously interfered with. When they recur after removal, it almost always happens that they are removed with more difficulty than at first. Seldom, very seldom, can a second or third growth be so neatly detached as the first one. I think the base usually spreads more widely with each recurrence, and the surrounding membrane is apt to become more and more changed.

In order, then, to carry out our plan efficiently, we must, of course, be able not only to reach, but also in some measure to command, the base of the polypus. The first step, therefore, is to define, as well as we can, its point of attachment. Now, in illustration of the extent to which it is sometimes necessary to proceed, let me ask your attention to the following case.

Ernest Scott, aged 13, apparently a healthy boy, but small

for his age, was brought to the hospital in May 1874, with a polypus in the nose and pharynx. The right nostril was tolerably free, but, when a director was carried along the left nostril, it came into contact with a firm foreign body at the back part, which prevented it from passing into the pharynx. The soft palate was pushed somewhat forward, and appeared unduly convex. When the finger was carried up behind it, a well-defined polypus could be made out, occupying much of the space behind the posterior nares, principally on the left side, and attached to the upper portion of the margin of the septum and the roof of the left side.

A few days after admission, the boy being under chloroform, a strong pair of forceps was passed along the left nostril, and, guided by the finger behind the soft palate, the blades were separated and carried on each side of the neck of the growth. This being firmly grasped and twisted, a large portion of the mass was torn away and brought through the nostril. A second and third attempt removed all that remained or could be detected, and the boy very speedily recovered from the operation. A week or two afterwards, a second and very thorough examination was made of the region, but no traces of the polypus could be discovered, and the boy left the hospital seemingly quite well.

Mr. Butlin examined the tumour, and described it thus: "Very firm and tough, presenting to the eye the appearance and to the touch the characters of firm fibrous tissue. Section streaked occasionally with red or purple. The masses were irregularly lobulated, apparently not encapsuled. Microscopical characters: sections, carefully examined, appeared to consist of wavy fibrous tissue, but mingled with round, oval, or even stellate cells in various proportion. The character of the connective tissue varied very much, even in different parts of the same section. Each of the polypoid growths was covered with a thick layer of tessellated epithelium, several cells deep." He adds: "It is probable that these tumours are outgrowths of the mucous and submucous tissues of the interior of the nostrils. No distinct follicles, lined with epithelium, were observed, although it might have been expected that adenoid structures would exist in them. It is not unlikely that they will recur."

We saw no more of the lad for a year, until May, 1875, when he returned, now fourteen years old, in a very sad state; and told us that he had been suffering from a recurrence of the disease for the last three or four months. He looked ill and had evidently lost flesh. It was at once evident that a large and formidable mass occupied a great portion of the left nostril, some part at the back of the right, and the whole of the upper region of the pharynx. The left nostril was visibly expanded.

On opening the mouth, tho soft palate appeared forcibly thrust forward. It was very convex and extremely tense, and below its free border the convex surface of a smooth firm pale tumour could be seen. This so entirely blocked up the naso-pharyngeal region, that the finger could not at any part be carried up to the side of it, nor could any instrument be passed far into the nostril. Moreover, the outline of the left side of the face differed from that of the right. It was fuller, the cheek was wider, and further examination showed that this depended on an extension of the growth laterally behind the left jaw and past the pterygoid plates, until it could be obscurely felt as an ill-defined mass below the zygoma. There had been no hemorrhage from the tumour, but it was apt to bleed a little when touched by a probe or other instrument in the nostril. There was no glandular enlargement. The local effects of this very ugly tumour were distressing enough. The poor boy swallowed with difficulty, he articulated indistinctly, he was very deaf, especially on the left side, and—gravest result of all—his respiration was embarrassed. This latter effect was gradually increasing. When first admitted, the respiration was noisy and laboured. During sleep, it became so difficult that he was continually awakened, and at length could only doze for short periods, and the noise he then made disturbed the whole ward.

Now, it was very clear that, if this recurrent polypus were left alone, the boy had not long to live; and the important question was, in what way an attempt should be made to remove it. Was there any chance of detaching it by the plan previously adopted—by any kind of manipulation through the mouth or nostril? Was it practicable to get sufficient command of the tumour this way, assisted by the division of the nostril, or soft palate, or both? We thought not. It seemed to us highly improbable that by this means I could get at the base: and, moreover, even when in far more favourable circumstances I removed it in this way, it recurred. Then what further should be done? The tumour was behind the left upper maxilla, and if I previously removed this, there was every probability that I should freely expose the mass. But would nothing short of this severe measure suffice? Might I not, as some have done in similar cases, cut away the back part of the hard palate, and thus get room? But the patient was so small and the mass so large, that it was probable that even this might prove inadequate, and it hardly seemed wise to attempt any means that did not offer a strong chance of success. Therefore, we decided to remove the bulk of the upper maxilla and the palate; not absolutely the whole of the left bone, but all of it except the orbital and nasal portions. But again the question occurred, might we not adopt a proceeding which has occa-

sionally found favour at the hands of some eminent surgeons on the continent? Might we not, short of completely removing the upper jaw, detach it from adjacent bones, turn it upward or aside awhile, get in this way sufficient space to deal with the tumour and then replace it, and so, after all, save this important bone? Well, the proposition was to me, in this instance, a tempting one. If I could only have done this, then, indeed, there would have been but little to qualify the advantage of the operation. But on considering the case in all its bearings, we came to the conclusion that I ought not to run the risk which it involved. In the first place, as before mentioned, I was loath to adopt any operation that did not offer a strong probability of success, and I doubted whether by this plan, after all, I should obtain sufficient room to reach the attachment of the tumour. Remember that in the child the maxilla is not only absolutely but relatively smaller than in the adult. Again, every fraction of space is important in this operation, not only in enabling one to command the growth, but because the avulsion of such a mass is, as we well know, liable to be followed by profuse hemorrhage, and, chiefly on this account, it was desirable to see as much of the region as possible. Lastly, time is a matter of moment in such an operation, and, therefore, I was anxious to avoid all unnecessary complication, to make the steps as simple as practicable. So on June 9th, I began by removing the upper jaw through a vertical incision made from the nostril to the lip, then carried upward round the ala to the root of the nose and outward below the margin to near the external angle of the orbit. The facial artery was thus divided at its distal extremity only, and no ligature was required. The soft and hard palate having been divided in the usual way, the body of the maxilla was cut through obliquely from immediately below the zygoma upward and inward into the nostril, and thus detached. Then passing my finger backward, I could just make out the neck of the polypus, but in order to command the morbid growth satisfactorily, it was necessary to remove the pterygoid plate and to divide the soft palate vertically through the upper three-fourths of its extent. After this had been done, the neck of the polypus, which was now well in hand, could be securely grasped with very stout forceps, and by two or three forcible twists and wrenches it was torn away. It was generally pyriform, and consisted of two principal and two or three smaller lobes, all pendulous. The largest lobe, of the size of a common egg, lay to the left and came out of the pterygo-maxillary and temporal fossa. As the tumour came away, an artery of some size, deep in the left side, no doubt the internal maxillary, bled freely. This was at once secured, and no other vessel needed a ligature. As soon as the tumour was removed, I could feel, denuded to a large

extent of its periosteum, the under surface of the body of the sphenoid and the basilar portion of the occipital. On this site, the stalk of the polypus was implanted, and hence it grew downward and then outward into the pharynx and behind the left jaw. I touched the surface of the bones with a hot iron, put a silver suture through the upper angle of the partially divided soft palate, and then accurately adjusted the flap of the integument.

The boy bore the whole operation remarkably well, and never for an instant appeared faint. He was sometimes only partially under the influence of chloroform, for while he was on the table before the operation there was so much difficulty of breathing that it was not thought prudent to push chloroform far. He lay very quiet after the operation, showing no signs of distress, and having taken a few teaspoonfuls of wine and beeftea, sank into a heavy, but natural, sleep. When I saw him in the evening, he was still sleeping soundly and naturally. The respiration calm and deep, and the pulse 110. As the sister said, No wonder, for he wanted sleep, having had, from the impediment to respiration, no long interval of sound sleep since his admission. During the last few days, especially, his respiration had been extremely embarrassed. On the night preceding the operation, he was suddenly aroused by a choking fit, and until he was enabled to sit up suffocation appeared imminent.

The boy recovered directly and rapidly, without any untoward symptoms.—*British Medical Journal, Jan.* 5, 1878, *p.* 3.

45.—ON THE USE OF GLYCERINE IN THE TREATMENT OF INTERNAL HEMORRHOIDS.

By Dr. David Young, Florence.

Two years ago my attention was directed to the use of glycerine in the treatment of hemorrhoids in the following manner. An elderly lady suffering from diabetes mellitus came under my care. To allay her great thirst I ordered a drink consisting of glycerine and water acidulated with lemon-juice. She drank freely of this for several weeks, and during the same period used glycerine in every case as a substitute for sugar. During one of my visits she asked if the glycerine was intended to act in any way upon the bowels. I replied in the negative, when she told me that since she had taken it she had been almost entirely free from bleeding piles, from which she had suffered for several years previously, and more or less continuously. At this time I was attending a young lady with advanced tubercular disease of both lungs, and whose *ill-health* was greatly increased by the discomfort and pain caused by a large internal dorsal pile, which came down constantly at stool and upon the slightest exertion,

and occasionally, at varying intervals weakened her by bleeding profusely. A radical cure she would not hear of, and indeed I would have hesitated in attempting it upon such a debilitated subject. Everything else had been tried, the bowels were regulated as far as possible by diet, frequent ablution with cold water was carried out, and various astringent applications were used, but all to no purpose. I resolved to try the glycerine, and ordered two teaspoonfuls to be taken in a little water morning and evening. The result has been most satisfactory. She has now taken it for more than a year, during which time she has not had the slightest trouble from the pile.

I am at present trying it in several other cases, but only three of them have been sufficiently long under treatment to warrant anything being said about them.

The first case is that of a weak, nervous man, aged fifty-four, who has been a *bon vivant*, and is now a confirmed dyspeptic, and has suffered very much for several years from bleeding piles. As he would not consent to have the hemorrhoids removed, I ordered him three drachms of glycerine in a wine-glassful of water morning and evening. Six months have now elapsed, and he has had no trouble whatever from the hemorrhoids, and in other respects he is in better health than he has been for several years.

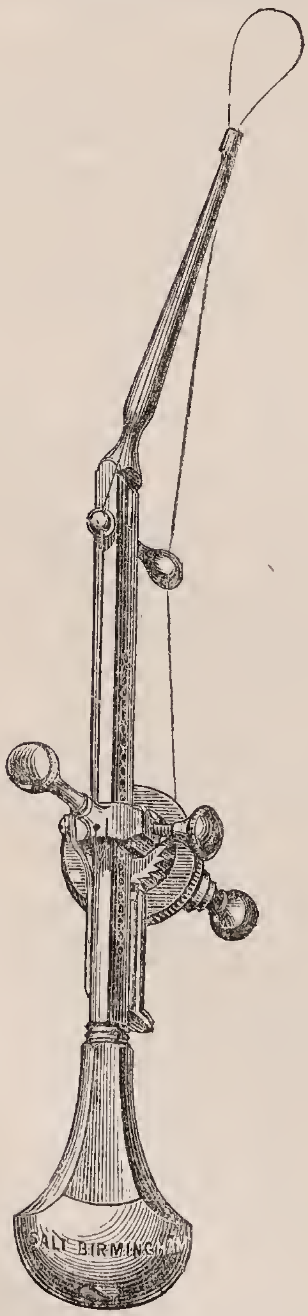
The second case is a young married lady. She has been tormented with bleeding piles for more than two years. On examination I found three large internal piles, and several flabby tabs of skin—the remains of external piles of bygone days. I advised her to have the internal ones removed by the thermo-cautery, to which she agreed, but her husband would not give his consent till every other means had been tried. She is now taking two drachms of Sarg's glycerine night and morning, and so far with a fairly good result. For more than two years she had never been free from attacks of pain and bleeding for more than a month at a time, and frequently not so long. Now she has passed three months with the minimum of discomfort and no bleeding whatever.

The last case I will mention is a young gentleman who has lived many years in Italy, and has suffered more or less from piles for two or three years. Sometimes the attacks have been very severe, compelling him to remain in bed for several days. Frequently he had complete prolapsus with the descent of the piles, and could never walk the shortest distance without feeling considerable discomfort in the rectum. In addition to the glycerine, he used at bedtime a suppository containing Persulphate of Iron, half a grain. Since he has employed these means, he writes that he is certainly better.

The foregoing cases seem to show that we may be able to add glycerine to our list of palliatives for this troublesome malady.

There are many patients who will not submit to surgical interference, and others—as for example consumptives in advanced stages of their disease—to whom one would scarcely recommend it, so that we are glad to welcome any means which would alleviate such a distressing condition. Not the least recommendation of this plan is, that it is both easy and pleasant, and probably also, especially in the case of phthisical patients, beneficial in some other respects. None of the patients to whom I have given it have experienced any difficulty in taking it, and when the sweet taste is an objection I usually order a little lemon-juice to be added to each dose.—*Practitioner, Jan.* 1878, *p.* 1.

46.—SNARE FOR AURAL AND NASAL POLYPI.



Messrs. SALT & SON, Birmingham, have made, at the suggestion of Mr. T. H. Bartleet, Surgeon to the General Hospital, Birmingham, a snare for aural and nasal polypi, which is an improvement on those at present employed. Those who are accustomed to the use of the polypus snare must have felt the inconvenience arising from the fact that the two movements commonly used—that is, the rapid one made by the fingers, or the slow one by the screw—can only be obtained by separate instruments. In Mr. Bartleet's snare, as will be seen by the engraving, *the two movements are combined*, so that the wire may be tightened by the fingers, working the cross-bar, in order to find and grasp the polypus, and then by the milled button, which gives a slow or écraseur action. The apparatus is simple in use, and calculated to prove a valuable aid in the treatment of nasal and aural polypus. As in other similar instruments, the shaft is jointed, so as to act at any angle.—*Lancet, March* 23, 1878, *p.* 426.

47.—ON STRICTURES OF THE INTESTINE; WITH REMARKS UPON STATISTICS AS A GUIDE TO DIAGNOSIS AND TREATMENT.

By Dr. Sidney Coupland, Assistant Physician, and Henry Morris, Esq., M.A., Senior Assistant-Surgeon to the Middlesex Hospital.

Nature of the Strictures.—In general terms, it may be said that the majority of the strictures are malignant, and this notwithstanding the fact that few are associated with secondary infection of remote viscera. This may be explained by reason of the new growths mostly belonging to the class of epitheliomata, which are notoriously the most local of all forms of cancer, and also possibly because they lead to fatal results before the system comes to be infected. In a case under our care last year of epithelioma of the sigmoid flexure, an annular ulcer with thickened infiltrated margins, and a depressed and ulcerating base, which gave to the gut externally the appearance of having been constricted by a tight cord, the epithelial new growth infiltrated the coats of the bowel for about half an inch above and below the constriction; but there was absolutely no secondary infection in any part. A case precisely similar as to the form of local disease in the sigmoid flexure, which was in the Hospital in 1875, was associated with small secondary nodules in the liver and in the cutaneous tissues of the umbilicus. We think, then, that even some cases which in past times have been recorded as instances of simple stricture may really have the same fundamental structure histologically. To the naked eye, they conform to the type of "annular ulcers," but microscopically they are composed of an exuberant growth of cylindrical epithelium in the deeper tissues of the wall of the gut, and frequently with small outbuds on the serous coat. In the cæcum, this growth may attain a larger size, and, as in the case above related, may form a large cauliflower excrescence, partially filling that portion of bowel. A smaller number of cases are due to scirrhous cancer, or to colloid; whilst there remain others, which from the presence of ulceration elsewhere in the bowel and the absence of any signs of infiltrating cancer, may be attributed to the cicatrisation of tubercular, dysenteric, or syphilitic ulceration; the latter, as is well known, being chiefly limited to the rectum. Analysing, as before, the sixty-three cases we have collected, we find them recorded or described as follows. Out of the thirty-two cases to be found in the Pathological Transactions, eighteen are recorded as cancer, eight as simple stricture, or stricture following ulceration; seven of these being in the sigmoid flexure, it is possible that some may have been of the nature of epithelioma. There is one case of stricture of the sigmoid

attributed to tubercular ulceration, and in two at the same region the nature of the stricture is not alluded to. There remain two instances of supposed syphilitic stricture of the rectum, and one attributed to cicatrisation of a dysenteric ulcer in the same portion of the canal. In the thirty-one cases recorded in the Middlesex Hospital register, twenty were undoubtedly cancerous; seven are described as simple strictures (but some of these, again, were in the form of "annular ulcers"), and four as "ulceration with stricture."

Perforation.—In the course of inquiry, we were struck particularly by the fact that in a large number of cases in which the obstruction was complete and where it remained until death, unrelieved by the operation of colotomy, the fatal issue was brought about by the occurrence of perforation of the gut either just above the seat of stricture or at the cæcum; and it is upon the latter complication that we desire especially to insist. Its occurrence in several cases under our own notice brought the fact forcibly before us, and has largely influenced us in advising a definite line of procedure. The occurrence of inflammation and ulceration of the intestines from simple accumulation of their contents is notorious. The history of ordinary typhlitis is an illustration of this; and, not to mention the, unhappily, too frequent instances of perforation of the vermiform appendix from impacted fæces, we could instance several cases in which the cæcum itself and other portions of the canal have suffered extremely from a like cause, even to the extent of perforation. In the class of cases we are particularly considering, the cæcum is usually the point to suffer most severely; but here again we proclaim no novelty; we only affirm that the fact has not perhaps received sufficient attention. Dr. Hilton Fagge refers to it in his paper—it was met with in four of his cases; and he adds further that Dr. Wilks "had seen, in his private practice, a case of stricture of the colon, which (although it never caused total obstruction) gave rise to accumulation in the cæcum, to inflammation and perforation of this part of the bowel, and consequently to the death of the patient." If we turn to our own experience, one of the most striking instances we have seen was in a female patient thirty-five years of age, who was admitted into the Middlesex Hospital under the care of Dr. Greenhow, on November 25th, 1873. She had been losing flesh for the past two years, and during the same time, had been subject to hypogastric pain, and more or less constipation, which had become pronounced only for twelve days. She had travelled up from Wales to enter a London hospital; and, on admission, she was evidently dying from peritonitis. She lived but six hours; and after death the obstruction was found to be due to cancer of

the body of the uterus invading the rectum; all the intestines were enormously distended with fæcal matter, and fæcal matter had escaped in large quantity into the peritoneal cavity. This extravasation was due to a perforation of the cæcum, the walls of which were extensively ulcerated. In 1874, a man, thirty-two years of age, with cancer of the rectum, also died from peritonitis due to perforation of the cæcum, there being no ulceration present in any other part of the greatly distended bowels. In 1876, two cases which came under our notice of annular stricture of the sigmoid, both in females, also terminated in this manner. In one, the cæcum was opened shortly before death; and in the other, the ascending colon; but in both, the operation was done too late to prevent the perforation of the cæcum, which, in one case at least, must have been established before the operation. Lastly, in the remarkable case of epithelial cancer of the ileo-cæcal valve already referred to, which allowed the entrance of fæcal matter into, but partially hindered its exit from the cæcum, there was extreme disorganisation of the walls of this *cul-de-sac*.

We need not here dwell upon these cases, but they all illustrate forcibly the fact of cæcal ulceration in cases of stricture even so far removed from the cæcum as the lower end of the rectum. The reason why the cæcum suffers in this way is not far to seek. In cases of simple accumulation, it is generally the chief part to be involved; and in cases of accumulation from organic stricture beyond, the same causes operate with increased force. These are, first, the shape of the cæcum, a mere *cul-de-sac*, above and on the inner side of which the ileum opens at a right angle. It thus acts as a kind of reservoir, where, in cases of obstruction in the course of the large bowel beyond, it serves as it were for the meeting of the two currents, that, namely, setting downwards from the ileum, and that regurgitated backwards from the seat of obstruction. Its dependent position is a second factor favourable to accumulation within it. More effectual still is its fixity, placed as it is between the abdominal wall in front and the iliacus muscle behind, and only partially invested by peritoneum, except in rare cases. Hence its power of independent movement is very slight; and that will be lost with the increasing distension. Contrast this with the transverse colon, which being, as a rule, more free, is enabled, so long as its muscular walls retain their tonicity, to empty itself. Further, to all this must be added the constant chafing of the distended gut by the action of the abdominal and iliacus muscles between which it is placed.

We are unable to give any accurate statistical details in support of our assertion of the frequency with which this cæcal ulceration occurs, owing simply to the fact that in a consider-

able number of cases the state of the intestinal mucous membrane above the stricture has not been put on record, or, if mentioned at all, the description is usually limited to the few inches of bowel in the neighbourhood of the stricture, except in those cases which have proved fatal from perforation. Eliminating twenty-six cases in which the state of the mucous membrane is not described, we find that ulceration occurred in the cæcum, or in its vicinity, and far removed from the seat of stricture, in fourteen out of thirty-one, a proportion which, judging from our own limited experience, is considerably within the mark.

Treatment.—We pass now to the question of treatment, which, we think, should be based upon the knowledge of these facts. Obviously, those cases of rectal cancer must be set aside in which the diagnosis by means of physical examination is easy, or in which the nature of the case has been made out long prior to the supervention of total obstruction. Of course, in such cases, the only rational procedure is that universally pursued by surgeons, viz., left colotomy; and all, in such cases, recognise the futility of delay and the long-lasting relief, for months or even for years, frequently obtained by that operation. But in all other cases, where the history is one of chronic obstruction, where the age of the patient favours the view of cancer, where, in fine, it is probable that a stricture of the bowel exists, then, without wasting time over repeated injections, administering powerful and harmful purgatives, we think that recourse should speedily be had to colotomy in the right loin. We advise this operation, because in a certain proportion of cases (about one-fourth) the stricture is higher than the sigmoid flexure; because in all these cases, whether the obstructing cause be far from or near to the cæcum, there is undue strain thrown upon that portion of the canal; and because the only chance of a favourable issue (in so far as an operation for relief of symptoms can be said to have a favourable result) obviously lies in giving prompt and early relief to the cæcum thus overstrained. It must be borne in mind that cases have occurred in which, the symptoms pointing to the disease in the sigmoid flexure, the descending colon has been opened and found to be empty, owing to the obstruction being situated in the course of the transverse colon or in one of its flexures. Had the operation of right colotomy been performed, not only would it have been above the seat of stricture, but it would also have at once unloaded the distended cæcum. The danger of delaying this operation cannot be too strictly insisted upon. Unfortunately, in the majority of cases, the surgeon is not called until the obstruction has been already complete for some time, and the cæcum has suffered in proportion. A case of stricture under our care last year, affords an

illustration of this. The patient, a woman fifty years of age, was admitted into the Hospital on October 10th, 1876, with a history of complete constipation of twelve days' standing, unrelieved by purgative medicines. There was great distension of the abdomen to about an equal extent on both sides, perhaps some slight fulness in the right iliac region. There was no vomiting. A long tube was passed *per anum* to the extent of twelve inches; warm water injected into it could not be heard on auscultation to find its way into the cæcum, and was speedily returned, without the passage of either fæces or flatus. When the patient was under the influence of chloroform, the hand was introduced into the lower bowel, without meeting with any obstruction except that which appeared to be a fold of mucous membrane. Right colotomy was performed about six hours after admission, and was followed by marked relief to the distressing symptoms of distension, &c., but the patient sank from peritonitis, dying thirty-six hours after the operation. The peritonitis—which must have been present on admission—was due to ulceration of the coats of the cæcum, leading to extravasation of its contents into the peritoneal cavity. The stricture was confined to the sigmoid flexure. In the twenty-third volume of the Pathological Transactions (p. 119), Dr. Bristowe records a case of colloid cancer of the splenic flexure in a young man twenty-three years of age, in which for some days injections appeared to give relief, and in which, at length, the operation of right colotomy was entertained. The patient died, however, a few hours before the morning fixed for the operation, and ten days after his admission into the hospital. There was perforation of the ileum close to the cæcum, the lower end of the small bowel being extensively ulcerated.

But we would go further than this. The operation of right colotomy may have failed in its main object, owing to the disease being situated in the cæcum or small intestine, for so difficult is the diagnosis in cases where the abdomen is uniformly distended that to ascertain the precise seat of a stricture is well-nigh impossible. The ascending colon may then be found collapsed and empty. In such a case, the wound in the loin should be stitched up, and relief afforded to the distended bowels by the operation of enterotomy, or the small bowel may be opened at the loin if thought desirable. The intolerable distress from fæcal and gaseous accumulation endured by the patient is too great to be let pass, without an effort on the part of the surgeon to remove its cause. Possibly he cannot hope to do much more than ease the path to death; but surely this is some gain. The desirability of performing this operation to give relief to over-distended intestines was insisted on by Trousseau (Lectures on Clinical Medicine, New Syd. Soc. Ed.,

vol. iv., p. 205), who, in his lecture on intestinal obstruction, gives directions as to the performance of enterotomy. He relates, also, four cases in which recourse was had to the operation at his suggestion, and as a final attempt, to relieve symptoms of obstruction. In two of these cases, the patients recovered, both being, probably, cases of volvulus or internal strangulation of the small intestines. In the Medico-Chirurgical Transactions for 1872 (vol. lv. p. 267) Mr. McCarthy relates a case of cancer of the stomach involving the splenic flexure, in which, on the advice of Mr. Maunder, he performed enterotomy in the right inguinal region, and with perfect success. The patient died from the cancerous disease and fatty degeneration of the heart, seven weeks after the operation. The operation has also been performed by Mr. Wagstaffe (St. Thomas's Hospital Reports, 1873, p. 181), for the relief of great distension and sickness from obstruction due to a pelvic tumour; and with such success that the patient was alive four years after the operation: and by Mr. Maunder (Clinical Society's Transactions, ix., p. 102), in a case of suspected stricture at the lower part of the ileum. The patient, who was sixty-eight years of age, lived for some months after the operation. It has also been performed by Mr. Bryant with a successful result. For ourselves, we may say that to us it seems that in all such cases, where the distension of the intestines is a source of suffering, as well as of danger, the operation of enterotomy is as imperatively called for as is that of puncture of the bladder in cases of overdistention of that viscus from impermeable stricture, and notwithstanding that fatal disease of the kidney may be already established. We hold that a free opening into the bowel is at once more effectual and safer than the method of acupuncture, which has been frequently adopted of late years. It is true that many may hold with Trousseau that puncture is not dangerous; but in a recent discussion at the Clinical Society of London, there appeared to be a pretty general consensus of opinion that acupuncture of the bowels was in many cases attended with considerable risk. Mr. Bryant said that in two cases in which he had practised it, fæcal extravasation resulted; and Dr. Silver, although urging the necessity of the procedure for the relief of overdistension, admitted that in one case, fæcal extravasation had occurred. In Dr. Bristowe's case of stricture at the splenic flexure, fatal from ulceration of the ileum, acupuncture was practised. It produced temporary relief, but seemed to determine the fatal perforation of the bowel. Certainly, this happened in the case of ileo-cæcal cancer under our care last year; for there numerous punctures were made into the various distended coils of small intestine, and were followed by a considerable escape of flatus, with slight

diminution of the girth of the patient. The operation was also, however, followed by increased violence in the peristalsis of the small intestines, with a proportionately great increase in pain, and the patient died from peritonitis due to perforation of the cæcum. Neither in this, nor in Dr. Bristowe's case, was there evidence of there having been any extravasation at the seats of puncture. There is more risk of such extravasation where the muscular coat has lost its tone and the walls are thinned by distension or spoilt by inflammation. This was well seen in a case of internal strangulation, under the care of Mr. Hulke, at the Middlesex Hospital, in 1872, and recorded by him in the Medical Times and Gazette (1872, vol. ii. p. 463). In this case, acupuncture was resorted to for the purpose of replacing coils of small intestine which had been drawn out of the abdomen in the search for the constricting band. The puncture allowing not only of the escape of flatus but also of the oozing of fæces, Mr. Hulke laid the bowel open at the spot and made an artificial anus in the small intestine. In Dr. Bristowe's and our own cases, the puncture appeared to be the exciting cause of the perforation of the ulcers produced by the fæcal accumulation, probably by exciting a more ready contraction of the small intestines, owing to the displacement of their contents. Had a free opening been made, however, it is probable that by thus allowing a vent for the escape of the fæces, the cæcum or the lower end of the ileum would not have been exposed to the additional strain caused by the increased peristaltic action. We have three times witnessed the great relief experienced by the operation of enterotomy in these cases; once in the case of Mr. Hulke, just referred to, and again last year in a patient of Dr. Henry Thompson (a case of sigmoid stricture), where the cæcum, already far advanced in ulceration, was laid open in the operation by one of us, and thus exit was given to an enormous quantity of fæces. In both cases—although the patient only survived for a few hours—the relief obtained was very marked. The third case was one of obstruction from intussusception, where twenty-four inches of ileum were removed, and a double artificial anus made. The patient lived twenty hours after the operation, and for many hours was made easy and even cheerful by it (Transactions of the Pathological Society, vol. xxviii, p. 131).

Diagnosis.—What we have to say upon the subject of diagnosis may be summed up in a very few words. We must confess, with Dr. Fagge, that so far as regards the methods employed for the accurate diagnosis of the seat of a stricture of the intestine, it is often impossible to be sure whether the disease is situated in the small or large bowel; and if it be in the large intestine, its precise situation there is extremely diffi-

cult to determine. All the rules based upon the symptoms of the disease have been at different times found wanting; and much the same statement may be made in truth with regard to such aids to diagnosis as are afforded by the amount of fluid that may be injected into the canal, by auscultation over various parts of its course during the injection and even by the passage of the long tube. In every case where digital examination has proved negative, it might be worth while to adopt the method first practised in this country by Mr. Maunder, largely employed by Professor Simon, of Heidelberg, and advocated by Mr. Walsham, of St. Bartholomew's Hospital; that, namely, of the introduction of the whole hand into the rectum. This method was employed in our case (above referred to) of stricture of the sigmoid, but owing to the folds of the canal, the stricture was not reached; and it was well that it was not, for at the necropsy subsequently, the bowel at the seat of stricture gave way on the slightest traction. Mr. Walsham also had failed in detecting a stricture of the sigmoid flexure by this means; so that, valuable as the method may be, it cannot be fully relied on, and, moreover, it should be practised with the greatest caution.

But, after all, from what has gone before, it will be seen that, in our opinion, the precise determination of the seat of stricture is not of primary importance. In determining this seat, we are thrown back upon statistics: a knowledge of these will be a valuable guide, and they should be taken into prominent account when forming a diagnosis. We know that three-fourths of the cases of stricture involve the rectum and the sigmoid flexure; and we know that, of the remaining one-fourth, a very small proportion are seated above the ileo-cæcal valve. Nor if we have arrived only at so imperfect a diagnosis as one based upon mere numerical averages, is our line of treatment at all the less secure. For we know, also, that the chief part of the passage to suffer from the effects of stricture of the large bowel is the cæcum; and we know that, if the ascending colon be opened, in 90 per cent. of these cases the opening will be above the seat of stricture, and will also give relief to the overdistended cæcum; whilst, as for the remainder—that is, those cases in which colotomy fails in its object—enterotomy should be performed and relief thus afforded, although it may be with but a very imperfect conception as to the exact locality of the source of obstruction.—*British Medical Journal, Jan.* 26, 1878, *p.* 122.

DISEASES OF THE SKIN.

48.—ON THE TREATMENT OF ACNE.

By Dr. Robert Liveing, Lecturer on Dermatology at the Middlesex Hospital Medical School.

Common Acne is a disease of the skin which is generally cured without much difficulty, but the severe and inveterate varieties are admitted by all to be both troublesome and obstinate. It is to the treatment of these latter that I wish especially to direct attention.

The foundation of all successful treatment depends on the promotion of a healthy action of the sebaceous glands, and the consequent prevention of comedones; therefore, all very soothing remedies, while they produce a temporary relief to the inflammatory symptoms, do not strike at the root of the malady. The basis of all treatment should be vigorous rubbing with soap and flannel, for friction with soap, more than anything else, prevents the formation of comedones, and consequently of the acne pimple. The following plan of treatment succeeds in a large number of cases:—(1) The face should be steamed every night by holding it over a basin of hot water for a few minutes. (2) The skin should then be well rubbed for five or ten minutes with soap and flannel, or a soft nail-brush may be used with advantage when the skin will bear it; the soap should then be sponged off with warm water. (3) When the face has been dried, a lotion, composed of half-an-ounce of precipitated sulphur, two drachms of glycerine, one ounce of spirits of wine, three ounces each of lime-water and rose-water, should be thoroughly applied and allowed to dry, and remain on all night. If the skin is greasy the addition of some ether to the lotion is an advantage. Sometimes an ointment is more effective than a lotion; in that case one drachm and a half of hypochloride of sulphur, ten grains of carbonate of potash, ten drops of oil of bitter almonds, and an ounce of lard may be used; or three drachms of sulphur ointment and five drachms of vaseline will be found to be a very useful unguent. Whatever is used should be allowed to remain on all night, and washed off in the morning with warm oatmeal and water or weak gruel. If the skin becomes very tender under this treatment, it may be discontinued for one or two nights and then resumed. The most common cause of failure is want of perseverance or timidity on the part of the patient or doctor, for a temporary increase in the redness and irritability of the skin often prevents the continuance of the most efficacious remedies.

The treatment I have here indicated will be generally successful in dealing with ordinary acne, but will fail in a certain

proportion of inveterate cases. In these I find nothing so effectual as the application of potash soap in the form of a lotion well rubbed on the skin every night. One ounce of soft soap, one ounce of rectified spirits of wine, and six ounces of rose water will generally be found of sufficient strength. The lotion should be applied with a piece of flannel, and vigorously rubbed on the skin for as long a time as is convenient, the longer the better, short of making the skin really sore. Then the lotion should be washed off, and one of the sulphur lotions applied and allowed to dry on. Sometimes, after the vigorous use of soft soap, sulphur is not easily borne; in that case the following lotion may be used instead:—Prepared calamine powder and oxychloride of bismuth powder two drachms of each, rectified spirits of wine half an ounce, glycerine one drachm, perchloride of mercury three grains, and rose water to eight ounces. The quantity of perchloride of mercury may be increased if necessary. The worst cases of acne will in time yield to the soft soap treatment.

With regard to general treatment of acne no rule can be laid down that shall be applicable to all cases. Any symptoms of dyspepsia or irregular menstruation may point to a rational plan of treatment, but in the absence of any indications of this kind we may hope to succeed by local means alone. It is not uncommon to find that people who suffer from acne are anæmic, or otherwise below the normal standard of health; in all such cases, iron, either alone or combined with arsenic, will be found useful. In severe cases of acne, associated with chronic dyspepsia, the oxide of silver (one grain in a pill) given twice or three times a day is often beneficial. I find empirically that small doses (ten to fifteen minims) of the solution of perchloride of mercury, in combination with the tincture of the perchloride of iron, or with tincture of chinchona, has a very good effect on many obstinate forms of the disease, even where there is not a possibillity of syphilitic taint.

In nearly all cases the local treatment of acne must for a long time be more or less continuous, inasmuch as when left to itself the disease is apt to return, but instead of using the remedies every night it will be sufficient if they are applied once or twice a week.—*Lancet, Jan.* 19, 1878, *p.* 83.

49.—COLOURED EXUDATES IN ECZEMA.

By Dr. W. Lauder Lindsay, F.R.S.E., Physician to the Murray Royal Institution, Perth.

About seven years ago (in 1871) I was for a time much puzzled with the blue, or occasionally green, discoloration of the dressings applied to the eczematous leg of a male patient,

then aged thirty-five. The patient in question has been continuously under my observation, as an inmate of the Murray Royal Institution, for ten years, and he continues to be so. He is a tall, handsome, athletic man, who has been subject to eczema throughout the period of his residence under my charge, and he was so for years previously. At the time spoken of (in 1871) the disease, which has all along appeared in periodic acute attacks, chiefly affected his legs, one (the left) especially, or both; and it had done so for some years previously, as it did for a year or two subsequently. It sometimes involved also one or both hands and the face. Now it never appears in the legs, but confines itself to the face; but it was only during one attack in the legs that the peculiar exudation that forms the subject of the following paper exhibited itself. During the attack in question, which extended over several weeks, the various dressings applied to the leg, as well as the drawers, stockings, or other articles of clothing that came in contact with the copious eczematous discharges, became saturated with these discharges, which assumed various shades of blue, sometimes of green, whether the material was lint, rags of cotton, or linen or woollen (woven). The colour of the exudate most frequently resembled the blue stains that might have been made on the same textures by solutions of sulphate of copper or of indigo. And so artificial did these exudate stains appear, that I at first suspected the surreptitious use of some unauthorised lotion. But it was utterly impossible that, in this case, there could have been any sort of deceit or trickery on the part either of the patient or his attendants, even had any motive for it existed; for the case happened in hospital, where it was continuously under my own eye, and the phenomena all but disappeared and then reappeared long after it had become evident that the exudation and its colouration were genuine pathological phenomena. At different times all shades and combinations of blue, green, and yellow were to be seen on cloths or clothes of different textures; and these colours were so far fixed that they were distinct and apparently unchanged months afterwards—for I kept the stained dressing cloths for a considerable time, in the hope of getting the colouring matter of the exudate chemically analysed. It may be added that the exudate varied in fluidity and viscidity as well as in colour. On a subsequent as well as on former occasions the eczematous exudate was yellowish, though of the same sticky, lymphy character.

At this particular time the patient was taking no medicine of any kind, while the local applications were simple water-dressings; there was no peculiarity in his diet, nor was there any in his urine. The latter was essentially healthy, both as to

quantity and composition: its specific gravity was 1020, and it contained small quantities of mucus or urates, according very much to temperature; for I commonly find, in the same persons in whom the urine is quite clear in summer, that it may become clouded or loaded with urates in winter. Eczema is probably quite as common among the insane as the sane; nor are the cases less inveterate. But in no other case have I met with such a colouration of the exudate; nor, in the single instance in which I did meet with it, did it appear more than once—that is, during one attack of the skin disorder,—notwithstanding that the patient's previous and subsequent attacks were apparently of the same character, beginning under precisely the same conditions of residence, diet, exercise, and so forth. But not quite under the same conditions as to drug-giving or applying; for, in prior attacks, in deference to the opinions of our best dermatologists, I had tried a variety of local and general remedies of the most diverse and opposite kinds, including both acids and alkalies, ointments, lotions, and dry applications. Benzoated ointment of the oxide of zinc with carbolic acid added; carbolic acid separately, but variously diluted; vinegar; and various salts of soda in solution, were among the topical applications tried, and tried, I believe, with no beneficial result, save in so far as they may have relieved itching or itchiness—a relief quite as obtainable by simple water-dressing. Arsenic was administered internally, and occasionally Epsom salts or seidlitz powders were used to counteract, by gentle purgation, the effects of want of exercise. Nor did this constitutional treatment produce any appreciable result. That is to say, the eczema came and went at certain intervals, unaffected by the medication that was being used.

This being the result equally of medication and non-interference, I had given up all drugging and all local applications, save simple water-dressing, at the time when the coloration of the eczematous exudates occurred. The patient was resting quietly in bed; had given up (here again in deference to the views of our leading dermatologists) for the time being the malt liquor he was in the habit of using, substituting therefor soda or potash waters, or lemonade, or using none of these drinks in the middle of the day. In my patient the eruption and its discharge had an obvious relation to irritation by scratching—so much so, that I believe but for this the eruption would never have attracted notice.

He had long been the subject of a certain degree of mental imbecility—the result, it was affirmed by his friends, of a breakdown at school at the age of sixteen; this the result, in its turn, of that "competition" and "cram" which now are so much (and too much) the order of the day in matters educational.

This imbecility is mainly of the kind that simply disables a bodily strong man for taking part in any of the usual affairs of life; for making his own way in the world by his exertions in business or in the professions. But, unfortunately, he is further disabled by recurrent attacks of mania, varying in duration and intensity. And it is interesting to note that there is a striking correlation between the attacks of eczema and those of mania, the former immediately preceding the latter; and this precedence being most marked when the eczema is facial, or affects also the scalp, which has little hair upon it.

Now, it may be supposed that the morbid mental condition of the patient could have had nothing to do with the chemical peculiarities of the eczematous discharge, with its blue or green coloration—and possibly it had not; but quite as possibly it had. For we know that mental conditions—that emotions or passions, for instance (especially such a one as anger, whose alliance with mania need not here be pointed out)—produce chemical changes in the milk and other secretions in other animals as well as in man. And these changes are sometimes of a very local or limited and puzzling kind. Thus, we read of a "difference in the milk of the two breasts of the same woman" at the same time—differences both as to chemical composition and quantity, so that the milk of one mamma was refused by infants, whilst that of the other was taken freely. It is not at all remarkable, then, that chemical differences should exist in other secretions or excretions in the same patient at different times, or in different patients under diverse conditions. Professor Laycock showed that certain morbid pigmentary changes are connected with emotional states; just as they are also with peculiar conditions of the genito-urinary organs.—*Medical Times and Gazette, March* 9, 1878, *p.* 247.

50.—PSORIASIS TREATED WITH PHOSPHORUS "PERLES" AND CHRYSOPHANIC ACID.

By Dr. Balmanno Squire, Surgeon to the British Hospital for Diseases of the Skin, London.

It is now scarcely ten months ago since I first brought chrysophanic acid under the notice of the profession as a remedy in psoriasis. In the Medical Times and Gazette of February 10 of this year I referred more fully to the subject. Since then the remedy has come into extensive use. The account I gave of the first trials of chrysophanic acid in psoriasis was of so surprising a kind, that others soon began to put statements apparently so extravagant to the test of a practical trial. It was easy to do so; and anything that would promise to cure so obdurate a disease within a short space of time was obviously

at least worth a trial which seemed to require so little exercise of patience and trouble. Several communications on the subject have accordingly since been made by different observers to the various medical journals. But if my original account of the effect of chrysophanic acid in psoriasis was rather a startling one, the further accounts of other observers have been no less so. Most of those who have tested my observations by repeating them in their own practice have expressed themselves as scarcely less astonished by its efficacy than I myself was when its effect first became unfolded to me. The last observer who has thus expressed himself is Dr. Whipham, who has quite recently recorded his experience. This observer relates that he at first treated a particularly obstinate case of psoriasis by such well-reputed means as "careful attention to the diet," including the special inhibition of beer, a course of arsenic, the use of nitrate of mercury ointment, then a mixture designed to stimulate the action of the kidneys, then another mixture designed to correct disorder of the stomach (if any), then Donovan's solution of arsenic and mercury, "until finally the gums showed evidence of mercurial poisoning." He further states that this treatment was carried on in the case unsuccessfully for the greater part of three years, until he at length made trial of chrysophanic acid, which cured the case in three weeks. He adds that "it was not without a feeling of despair that I had recourse to chrysophanic acid. The result, however, and the rapidity with which that result was brought about, surprised me extremely—a surprise which is not lessened by the fact that the girl had suffered from the skin disease for five years and a quarter at the time when the acid was employed, and that she was entirely free from psoriasis in twenty-one days." He concludes with the remark that "at present our knowledge of the drug is limited, and recorded cases are few in number." I need, therefore, scarcely apologise for adding another case to the number, the more especially as it illustrates the means of avoiding the more important of those drawbacks to the remedy which, in some of the commentaries that have been made on my views, have been particularly insisted on.

A gentleman, aged sixty, affected with psoriasis, was recently sent to London to be under my care, by his medical attendant, Dr. James Adams, of Glasgow. Our patient had been affected with his disease continuously for the past twenty-seven years, and had latterly become anxious about it. He had begun, as he said, to think that "this thing would never leave him," and he had in consequence got to worry himself very much about the matter. Dr. Adams sent me a complete account of this gentleman's history, so that I was enabled at once to learn that he had experienced a battery of well-directed skill, which would

only a year or two ago have convinced me that I had nothing to cap it with. The patient was affected with psoriasis in a fairly copious manner over his scalp and forehead, his belly and back, and his forearms, thighs, and legs. Some of the patches, more especially those on his loins and his forearms, were of very considerable size, and the eruption annoyed him by occasioning him considerable irritation. After inspecting him, I decided to treat him with chrysophanic acid ointment and phosphorus *perles*. Dr. Adams had given me a *carte blanche* to treat him in any way I pleased, with the solitary proviso that I was to cure him if I could. He had directed his patient to remain in London until I was in a position either to say that he was well or that I had found myself baffled. It is not often that so free a course and such ample discretion are offered to anybody. I knew perfectly well that such double-edged weapons as phosphorous internally and chrysophanic acid externally were not without their risks, but I determined to push both of them to extremity with all due speed, so that I might yet have time to resort to other means if they should chance to disappoint me. The full effect of phosphorus in psoriasis, I knew by previous experience, would take a month to assert itself. The effect of chrysophanic acid would, as I knew quite positively, be declared, for yes or no, within a week. My patient was, as is the rule with patients affected with psoriasis, a remarkably robust and healthy man. The firmness of his fibre, his clear ruddy complexion, his bright eyes, and his hearty demeanour, were sufficient justification for the trial of "heroic" measures. He was moreover a man of considerable intelligence, and, as he happened to be a chemical manufacturer, he at once fully appreciated those precautions which are essential in the employment of chrysophanic acid ointment. His scalp and forehead had to be treated in *some* way; and the ointment when used to the scalp, and more especially the forehead, in my own practice, as well as already in the practice of others, has occasionally come to inflame the eyelids and conjunctivæ, and to give rise in some cases to marked œdema of the lids. I accordingly enjoined the use of benzol (the best solvent of the ointment) as a means of freeing the fingers completely from the latter after applying it, and I recommended furthermore the wearing of a nightcap, to avoid the smearing of the ointment at night on to the pillow, and so possibly on to the eyes.

The patient was treated with chrysophanic acid ointment of the strength of two drachms of the acid to the ounce of lard, the former being fully digested in the latter at a temperature of 360° Fahr. (oil bath), to insure the requisite incorporation of the acid with the lard.

The phosphorus, exhibited in the form of "perles"—that is to

say, the little capsules containing each one-thirtieth of a grain of phosphorus dissolved in oil, which may be obtained of Messrs. Corbyn, or, indeed, as I believe, of almost any chemist,—was regulated in the first instance to one-tenth of a grain per diem, but was speedily increased to rather over a third of a grain per diem, which proved to be the greatest dose that the patient could tolerate without experiencing gastric pain. The patient was taught to soften his scales efficiently with soap and water, and then to remove them thoroughly by scraping them away with a dull-edged knife before each application of the ointment.

I began treatment on September 24, 1877. By October 1 the greater part of the eruption had disappeared. Certain portions, however, of the disease—that is to say, the circumferential part of a good many of the patches (the most difficult of all in every case of psoriasis to cure)—proved themselves to be unwontedly obstinate. However, on October 26 the patient left London for Glasgow in a condition of complete freedom from his long-experienced disease. Dr. Adams is well known as one of the examiners of the Faculty of Physicians and Surgeons at Glasgow, and I therefore quote from a letter he wrote me on October 30, 1877, as a sufficient authority for the nature of the result. He says:—"I saw Mr. —— on the day after his return. I had him stripped, and overhauled him in a state of surprise that gave him intense delight. I am truly astonished. He has had the disease now for twenty-seven years, and, though there have been ups and downs, you got him at the worst." In reporting this case I have deviated from a custom that I prefer as a rule to follow. It is therefore necessary that I should say that I have in my previous papers, herein referred to, contributed the results obtained by me in the treatment of psoriasis by phosphorus used separately, and by chrysophanic acid used separately. It is only on the data acquired by the separate use of these two remedies in this disease that I have ventured, as here, to employ them in conjunction. I must not omit to say that the patient succeeded by the means I have named in avoiding any irritation of his eyes which might otherwise have arisen from the use of the ointment, and that although my desire to acquit myself speedily of my task led me to pushing the effect of the ointment to some erythematous inflammation of the skin, this was, by due care, always kept within fair bounds. The temporary yellow discoloration of the patient's hair occasioned by the use of the ointment was readily removed (when the time arrived for doing so) by means of a weak solution of caustic potash. And, lastly, I must admit that his under-linen was irretrievably stained by the acid of a dingy purple colour. But he, good-naturedly, made light of

this annoyance. His only real grievances throughout were that his hair, which properly was white, had become temporarily converted by the colour of the ointment to a brilliant yellow colour; and for the rest, that for one night only (this was at the climax of treatment by the ointment) the tingling sensation awakened in his skin kept him awake.—*Med. Times and Gazette, Dec.* 8, 1877, *p.* 615.

51.—THYMOL AS A REMEDY IN SKIN DISEASES.

By Dr. H. Radcliffe Crocker, Assistant Medical Officer to the Skin Department, University College Hospital.

Ten years ago, a paper was sent to the Pharmaceutical Society by a M. Bouilhon, a French chemist, advocating thymol as a substitute for carbolic acid (Pharmaceutical Journal, January 1869). Since that time until lately, this substance has attracted but little attention in England; but in Germany, several have worked at it, among whom may be especially mentioned Volkmann, who has used it in antiseptic dressings, after Lister's method, instead of carbolic acid spray; and Bucholtz of Dorpat, who has made experiments upon its power of preventing or destroying living organisms in fluids, in comparison with other antiseptics. In this country, however, it has been recently brought into general notice, mainly by the lectures of Dr. Burdon Sanderson at the London University "On the Infective Processes of Disease;" and as I have been employing it for the last five months in several skin affections, it will be opportune to relate my experience of it. I was first shown this substance by Mr. Martindale of New Cavendish Street, who was selling it, dissolved in vaseline, chiefly as a lubricant for the finger in obstetric and similar examinations. Finding that its properties were similar to those of carbolic acid in some respects, I thought it would be useful as a stimulant application in skin-diseases, and so far it has certainly exceeded my expectations.

As when prescribing it I am constantly asked what is thymol, and the information regarding it is scattered through many publications, it may be well to give a short account of it. It is obtained from the essential oil of thyme, which is found in several plants—*Thymus vulgaris*, *Thymus serpyllum*, *Mentha sylvestris*, and *Ptychotis ajowan*, the last a very common plant in India, which would probably be the main source if much demand arose for it. Oil of thyme consists of two bodies; one a liquid hydrocarbon, thymene, and the other oxidised, thymol ($C_{10}H_{14}O$). It is placed by chemists in the camphor group, and is homologous to phenol, forming thymolates and sulpho-thymolates with alkalies. As imported into this country, it is

a white solid, crystallising in oblique rhombic prisms, though from weak solutions it may be obtained in the form of needles, with the odour of oil of thyme, and is obtained in the solid form by freezing the essential oil or by distillation ; but when made by acting on the oil with caustic alkali, with which it combines, and separating it from the alkali by an acid, it occurs as a liquid which cannot be made to crystallise. In water it is only permanently soluble about one part in a thousand, but readily soluble in alcohol, ether, glacial acetic acid, vaseline, and fatty substances generally. Still better as solvents, both on account of the quantity taken up and because so dissolved it can be diluted to any required extent, are the caustic alkalies, thymolates being formed, I believe.

As I wished to use it in the form of lotion as well as ointment, Mr. Gerrard, the dispenser of University College Hospital, undertook to work out the pharmacy of it. The results, of which I have availed myself in prescribing, are as follows. Five grains of thymol dissolved in an ounce of rectified spirit will not be precipitated on the addition of an equal bulk of water ; but some will be thrown out when diluted to four ounces to be redissolved when the proportion of six ounces of water to one ounce of spirit is reached. In the proportion of two grains to the ounce of spirit, it is miscible with water in any proportion. A solution of seven grains of caustic potash in a drachm and a half of water will take up fifteen grains of thymol. A solution of ten grains of caustic soda in a drachm of water takes up thirty grains of thymol. Glycerine only increases the solubility in water very slightly. Further details may be found in Mr. Gerrard's paper in the Pharmaceutical Journal.

As an outcome of the above, I have used the following formulæ.

1. An ointment, consisting of one ounce of vaseline and from five to thirty grains of thymol ; the thymol being dissolved in the vaseline.

2. A lotion, consisting of thymol, five grains ; rectified spirit and glycerine, each one ounce ; water, sufficient for eight ounces. Tha glycerine is added to correct the desiccating effect of the spirit alone.

3. A solution of five to eighty grains of thymolate of potash in eight ounces of water.

As yet, I have not had occasion to use stronger lotions than the above. I have only lately used the last lotion ; but so far, have found it equally efficacious, while it has the advantages of economy and the readiness with which the strength may be increased. Ointments made with lard instead of vaseline act very well, but vaseline ointments have a better appearance.

The disease in which I first prescribed it and have had the greatest success is psoriasis.

In my early cases, I used the ointment of a strength of twenty-five grains to the ounce, to be rubbed into the seat of eruption after the removal of the scales, night and morning; but I soon found that it was a powerful stimulant, and that 5 per cent. was too strong for many cases; and here I must dissent from Husemann (quoted in the Year-Book of Pharmacy for 1876, p. 283), who has made some physiological experiments with this drug. He says: "It produces no irritation on the skin, but it does on the lips." In a concentrated form I have found it even caustic in its effects. In a case where the ointment had been carelessly dispensed, so that crystals were present undissolved in the vaseline, minute holes in the skin were produced in those parts where the crystals had remained some time. The discrepancy arises from the insolubility of the drug in water; when quite dry, Husemann is perhaps correct, but, in the presence of a solvent like vaseline or the alkaline saliva on the lips, its irritant effects are apparent. To return to the psoriasis, I found it better to begin with a weaker ointment, namely, ten grains to the ounce; and then, if the remedy were suitable, to continue as long as improvement was manifested, and if it became stationary, to increase the strength by five grains to the ounce until, in some cases, thirty grains to the ounce was reached. In the majority of cases, the weaker ointment was sufficient to cure the case; and another advantage is that it can be more continuously applied than the stronger forms, a method to be preferred, as a rule, to intermittent applications. Many cases treated with thymol showed rapid improvement, and some very chronic cases, which had resisted other treatment, including tarry applications, improved, and were finally cured by it.

If the disease were limited or nearly so to its usual situations on the extensor surfaces of the forearms and legs, I usually ordered the ointment; but when the diseased surface was of considerable extent, a lotion was prescribed to be applied with soft rag several times a day, lotions being generally more convenient in the daytime to people following their usual avocations. In some people it produces tingling and occasionally smarting when first applied, but this only lasts a few minutes. Like all stimulant remedies, it does not suit every case, and must not be applied, or at least very dilute, when, on removing the scales, the parts are much hotter to the touch than the surrounding skin and very red; in short, whenever the hyperæmia is considerable. This must be first subdued by soothing astringent measures externally, and appropriate internal medication, and then thymol applications will materially hasten the cure. In

fact, it is most successful in that class of cases in which tar is usually prescribed, and while quite as effieacious and in some cases succeeding where tar fails, it is cleaner, colourless, and hence can be used on the face without producing the brown discolouration of oil of cade and other preparations of tar, while the odour is rather pleasant than otherwise.

In the later stages of eczema it is also extremely useful; some cases of very long standing, which had been submitted to other treatment of various kinds, rapidly yielded to thymol. It was necessary in eczema to use a weaker ointment of only three to five grains to the ounce; and I have not met with any case of eczema that required a stronger application than that, and unctuous are generally better than watery applications in this disease.

As might be anticipated, it is adapted to a smaller proportion of cases than psoriasis, and must be restricted to cases in the dry stage or where the amount of discharge is diminishing, *i.e.*, not until the activity of the inflammation has subsided; hence it happens that even in the same patient it would cure one part, and be too stimulating for another part where the inflammation was still active. If, however, due discrimination be employed, the duration of the disease may be much curtailed. Smarting when first put on is rather more frequent than in psoriasis. With similar precautions, it also rapidly completes the cure in so-called lichen agrius; but usually a preliminary soothing treatment is required for some time before thymol is prescribed.

Lewin and Bucholtz have shown that thymol is about eight times as powerful as carbolic acid as a destroyer of the lower forms of life, and hence its usefulness in vegetable parasitic diseases was suggested. Accordingly, I have treated cases of tinea versicolor, tinea tonsurans, and tinea circinata. In the last two I have not yet used it sufficiently to warrant an opinion as to its merits, but in tinea versicolor I have used an ointment of ten grains to the ounce and the thymolate of potash lotion of ten grains to eight ounces. The ointment was effectual, but slow in its action; but the lotion cured cases where a large surface was affected in a few days. I cannot, however, claim for it any great advantage over sulphurous acid and the hyposulphites. I may also mention, for what it is worth, that a case of lichen planus which has lasted five years, after a fortnight's treatment with thymolate of potash, shows more improvement than I have ever seen in so short a time; the itching is gone, and the eruption is less prominent.

I think we may conclude from the above facts that thymol is a valuable addition to the list of stimulant remedies for diseases of the skin, and probably also as a parasiticide for diseases

of fungous origin; but, like all stimulants, it must not be used wherever there is much hyperæmia, as it will be more likely to aggravate than benefit such active cases; judiciously employed, however, it gives results which cannot fail to be gratifying to prescriber and patient, while its pleasant appearance and odour, as compared with preparations of tar, with the avoidance of the discoloration of the hair and skin produced by chrysophanic acid, are not the least of its claims to attention.

Other therapeutic uses to which it has been put are, as a caustic to the exposed pulp of carious teeth, for which it is much used by French dentists, and as an inhalation in throat-affections; but I do not know how far it has been advantageous. I am not aware of its having been given internally, but Husemann is quoted in the Year-Book of Pharmacy for 1876 as having injected two *grammes* (thirty grains) under the skin of a rabbit weighing one *kilogramme*, the only effect being a slight decrease in the number of respirations and temperature and a slight increase of pulse, but probably the greater part was not absorbed. The urine was turbid, had a smell of peppermint, and contained blood-corpuscles and albumen. *Post mortem* there was marked accumulation of fat in the liver, and the kidneys were inflamed. The symptoms of poisoning by it were great irregularity of breathing and paralysis setting in gradually. The heart continued to beat after all other action had ceased. So far, that would not appear to promise much from its internal administration. Its great pungency also would be an obstacle to its being given by the mouth.—*Brit. Med. Jour., Feb.* 16, 1878, *p.* 225.

ORGANS OF URINE AND GENERATION.

52.—PHOSPHATIC DEPOSITS FORMED IN THE BLADDER; CYSTS; PHOSPHATIC CONCRETIONS.

By Sir H. THOMPSON, Bt., Surgeon to H.M. the King of the Belgians; Consulting Surgeon to University College Hospital; and Emeritus Professor of Clinical Surgery.

Deposits of "phosphatic matter"—that is, of salts of lime and phosphoric acid, of magnesia and ammonia and phosphoric acid, but in various forms and degrees of mechanical admixture—frequently appear in the urinary passages, especially of elderly men, and often occasion serious and long-existing trouble.

In the large majority of instances phosphatic salts are produced in the bladder itself; in a few they descend from the kidney, as in the simple deposit of amorphous earthy phosphates, at any age, in urine which is secreted of alkaline reaction. Occasionally, also, a small phosphatic calculus is formed in the kidney, but this is by no means a common occurrence.

Even then there is generally a uric-acid nucleus, although it may be minute; for I think there is reason to believe that when the phosphatic salt occurs in the form of renal calculus, it is rarely a primary deposit, but results from irritation of the mucous membrane there, somewhat as it does in the bladder under similar circumstances.

My object is to treat only of those deposits of phosphatic calculous matter which have their origin in the bladder, and which therefore, to save circumlocution, may be distinguished briefly as "cysto-phosphatic." They play a very important part in the history of stone in the bladder; and though no absolute line of demarcation can be drawn between the class of cysto-phosphatic deposits and the class of fully developed phosphatic calculi—a large example of the former being equivalent in most particulars to a small example of the latter,—it is highly desirable on many accounts to recognise that there i s distinction between them. It is partly, then, with a view o calling attention to this distinction, and of emphasising it when made manifest, that I propose this term to designate the class in question.

Among elderly men who are compelled to pass much of their urine, or all of it, by catheter, and occasionally when this condition does not exist, the bladder often becomes the seat of phosphatic precipitates. These may be passed as greyish-white granular particles, or in little masses, or they may be retained and form "concretions" of the size of a pea or larger, in any case producing more or less painful and frequent micturition. The issue of such a case, if unrelieved, is never doubtful; a phosphatic calculus is certain to be formed, and, if not removed, becomes large. Indeed, no calculous product is deposited so rapidly as this; consequently there is none which attains so great a size if its progress be not checked. The well-known mass in the museum of the Royal College of Surgeons, No. H 2, is a striking illustration, among many others, of this fact.

But these deposits should be removed when first formed by washing out the bladder, or, if this is not successful, by the flat-bladed lithotrite. Nothing is easier, more simple, or more safe in execution than the accomplishment of this in their early stage; the prevention or cure of the diseased action which produces them is another subject, to be presently considered. At any rate, they should be mechanically removed before they have any claim to be considered as "stone," which they will certainly become if they are let alone. In every case in which such removal is accomplished we have the satisfaction of knowing that the production of a stone has been avoided. And were the lithotrite capable of doing nothing more than rid the bladder of these deposits, it would still hold a high rank among the resources of surgical art.

I know it may be said, If such deposits cannot be removed by washing out the bladder, but require the aid of the lithotrite, and, moreover, must inevitably lead, if unchecked, to the formation of stone, why should they not be nosologically regarded as examples of "stone in the bladder"? I reply that, in order to preserve a clear understanding of the terms used among surgeons, a somewhat more defined meaning to the term "stone" has become necessary, especially since the introduction of lithotrity. For instance, every example of calculous material which requires the operation of lithotomy for its removal may be permitted, without question, to rank as "stone;" but it ought to be equally clear that every example of calculous material for which a lithotrite should be employed cannot therefore necessarily be admitted to the same category. Thus, the calculous material may be composed of loosely cohering crystals, like a small fragment of frozen snow, or may have merely a consistence like that of mortar, and yet may resist the mechanical effect of a current of water or of a chemical solution to remove it. In such circumstances the lithotrite is the best agent in dealing with these formations, although their physical qualities are not such as to make "stone" a fitting or descriptive title. It would be better to apply to the deposit in either of these two forms, the word "concretion;" and the designation "cysto-phosphatic" will indicate clearly their origin and nature. The term "cysto-phosphatic concretion" will thus exclude, on the one hand, the ordinary "gravel" of uric acid and oxalate of lime and the few phosphatic calculi which descend from the kidney, and, on the other hand, all hard "stones," whether urate, oxalate, phosphate, or mixed, which commence as gravel, and have been more or less augmented in size by accretion in the bladder. It will therefore comprise all salts of phosphoric acid with earthy and volatile bases formed within the bladder, and deposited as a more or less loosely-cohering precipitate, but not in the form and size, or with the hardness, of "stone."

Of course, then, the simple proceeding involved in the removal by the lithotrite of cysto-phosphatic concretions is not to be regarded as an operation for crushing the stone. I have certainly removed them some scores of times, for patients in the conditions described, but have never reckoned them as cases of lithotrity. To do so would be to magnify their importance, to the great prejudice of truth in surgical records and in relation to other matters not less important.

I may remark here that the formation of concretions occurs sometimes as a sequence to lithotrity, especially if much and long-continued inflammation has followed the operation, and they may and do occur after the same action, when there has been no lithotrity. They are often mistaken for fragments left

behind by the surgeon after the last-named operation, or for the "re-formation" of the stone. Their occurrence quite as frequently after the removal of the uric-acid stone as after a phosphatic one disproves, in many cases, this view, which may be called the popular one. But they are really the results of inflammatory action, and there is not a little interest attaching to the pathological history of some of them, as I think will appear.—*Lancet, Jan.* 12, 1878, *p.* 44.

53.—THE PATHOLOGICAL HISTORY OF CYSTO-PHOSPHATIC DEPOSITS.

By Sir Henry Thompson, Bart., Surgeon-Extraordinary to H.M. the King of the Belgians, &c.

It is well known that when, by reason of prostatic obstruction, atony or paralysis, the bladder is unable to empty itself by the natural efforts, the urine thus habitually retained becomes, after a certain time, altered in character, often offensive in smell, and no longer clear in appearance. In other words, its natural primitive elements have been rearranged, and new organic products have been added. What has taken place may be thus briefly described: the urea is broken up, ammonia is formed, the phosphates of lime and magnesia decompose; their earthy bases being precipitated; and from these actions are produced the ammoniaco-magnesian, or "triple" phosphate, as well as some bibasic salts. Associated with these products, in such a specimen of urine as that referred to above, mucus in abnormal quantity and pus are always present.

Every surgeon of experience knows that a man of advanced age may not once have emptied his bladder during several years, and that he may nevertheless lead an active life, subject to no other annoyance than that of frequent calls to micturate; the capacity of the reservoir being impaired exactly in proportion to the quantity of fluid abnormally retained therein. Notwithstanding this defect, the urine passed by this man may be perfectly clear, showing no sign of decomposition or trace of precipitated phosphates. The one thing I have referred to has been absent—viz., inflammation of the mucous lining of the containing organ. Let this once occur—and it may arise spontaneously, or it may be produced by the first catheterism, however carefully done (the mechanical disturbance of the bladder sometimes suffices in the condition described to set up slight inflammation)—and decomposition of the urine begins to take place, and triple phosphates may soon be observed. The catalytic agent, whatever it is, seems to be in some way connected with the appearance (as prelude or sequence?) of inflammation, although the association of the two is by no

means an invariable occurrence. It has been said that the introduction of germs by means of the catheter is a common cause of the decomposition. I do not say that they may not sometimes be agents in effecting this change, but I have good ground for denying that their presence is a frequent, still more that it is the chief, cause in these cases. Thus inflammation, followed by decomposition, may arise, and sometimes does so when no instrument has been passed, not unfrequently after some other mechanical disturbance: internally, as by the descent of a calculus, or externally by violent exercise, &c. An inquiry relative to the cause of cystitis is, however, not an essential part of the subject now under consideration. When these changes in the urine have taken place the secretion itself becomes a source of irritation, aggravating existing inflammation, or rendering it chronic, and it is to prevent the continuance of this morbid action that the employment of a catheter is necessary.

But a point of some importance to be noted in relation to cysto-phosphatic deposits is this—that little or no obstacle exists to their escape from the bladder when they first appear. To be more explicit; a patient may continue to pass easily and freely all the phosphatic salts precipitated in the bladder for a long period of time, and this, too, even in the presence of some chronic cystitis. There appears to be no risk of aggregation and concretion of the deposit for a considerable time, nor perhaps will there ever be if proper precautions are taken at the outset and continued. The following are the conclusions I have arrived at in relation to this subject:—

1. That, in its healthy condition, the bladder rarely, if ever, retains, but on the contrary expels, all phosphatic deposits.

2. That when the bladder is not healthy, but affected by chronic inflammation, provided it is not considerable nor very prolonged in duration, the power of expulsion is still almost as great as in the healthy organ.

3. That there is a diseased condition of the inner coat of the bladder, in which its ability to expel phosphatic deposit is almost lost, and in which the formation of concretions—and if these are neglected, of stone—is certain to occur. It by no means infrequently happens after cystitis that the mucous coat acquires a morbid condition, which is not so much one of actual inflammation as the result of long continuance of that action. The membrane loses its polish, usually in one or more circumscribed spots, and becomes abraded, roughened, even flocculent, and exudation of lymph sometimes takes place on the surface. This matter, which is extremely tenacious, and to which phosphatic salts strongly adhere, is wholly different, it need hardly be said, from the ordinary and well-known viscid mucus of the

bladder. The latter has often been regarded, not without apparent reason, as a mechanical agent for gluing together crystalline particles to form concretions, although I doubt that it acts thus to any considerable extent. The lymph exuded from an abraded spot becomes loaded with phosphates, attaching them to the surface beneath, from which the tenacious mixture is not easily removed.

Let me illustrate the action which takes place thus. Every hospital student knows that if a new gum-elastic catheter be fastened into the bladder and left there, the urine being healthy, no phosphatic deposit will occur during the first day or so, on the small portion of the instrument which protrudes within the cavity of the organ. If, however, the catheter remains for a considerable period, whitish granules will appear on its surface at different points, and these in time coalesce and form an enveloping crust. What is the *rationale* of the action ? Simply this: while the surface of the catheter was smooth and polished, no phosphates appeared, but after the urine had partially dissolved and abraded the varnished surface, making it slightly rough, and beginning to expose the fibrous basis of the instrument (a process which takes place much more rapidly in ammoniacal than in healthy urine), the roughened surface determines the precipitation of the salts upon it. The same thing occurs on the surface of a calculus as long as it is retained within the cavity of the bladder, the urates or oxalates being deposited while inflammation is absent, phosphatic salts when that change has taken place. And thus it is that in the rings of a cut calculus may be seen the history of a patient's troubled life during the period of its formation, quiet intervals showing one form and character of deposit, attacks of inflammation marked by a white phosphatic ring, and so on. Another illustration is furnished by the absence of deposit on a well-made india-rubber catheter, although retained for a long period. Its surface resists the action of urine ; if, therefore, the instrument was smooth at first, no precipitate takes place. I have left, in very exceptional circumstances, such a catheter for six weeks without removing it, and then found it as free from deposit as when introduced.

Now, these phenomena explain the reason why cysto-phosphatic deposits in some cases obstinately persist and recur. So long as the mucous lining of the bladder retains its polish, so long as no serious denudation of epithelium takes place, no precipitated phosphates, as a rule, will be detained in the interior, provided the organ can empty itself, or can be emptied artificially. No adhesion of phosphatic material to the wall will result at any point, and consequently the appearance of deposit is not in these circumstances a very serious matter. But

—and we shall see hereafter how closely this subject bears on the operation of lithotrity—the moment the mucous coat of the bladder has notably lost in any one spot its natural polish, has become denuded or roughened, so soon there is danger that phosphatic salts will be attached to that spot, and become the fertile and continuous source of concretion-formation in the bladder. If degradation of the tissues has gone far enough to permit the exudation of lymph, the condition approximates to that of ulceration, which, however, is rare in the bladder, and only present in very severe or long-continued disease. An illustration of the action which takes place in such circumstances may occasionally be observed after lithotomy, when the urine deposits on the surface of the wound a phosphatic coating which adheres sometimes with great tenacity to the exuded lymph there, while no deposit whatever occurs in the bladder itself. In those somewhat rare instances of calculus partially encysted in a sac of the bladder, the small rough surface which is exposed in the cavity acts in a similar manner. Phosphates are deposited upon it, and when the aggregation has assumed a certain size, it is detached, falls into the bladder as a concretion; the process goes on at the original spot, and may be repeated again and again.

I need hardly say that these statements are based on several observations of my own, made from time to time not only on the living but also on the recent subject.—*Lancet, January* 19, 1878, *p.* 82.

54.—ON THE MEANS OF PREVENTING THE FORMATION OF CYSTO-PHOSPHATIC DEPOSITS.

By Sir HENRY THOMPSON, Bart., F.R.C.S., &c.

The operation of lithotrity is occasionally followed by chronic cystitis with painful symptoms, and by frequently recurring production of cysto-phosphatic deposits. This condition may persist for a long period, and it may sometimes never wholly disappear. The numerical proportion of these unsatisfactory cases to those which are wholly successful is happily small; and even that may probably be diminished by the exercise of judgment on the part of the operator, and by his conformity to certain rules in operating. There are two points to which it is necessary to pay special attention in order to avoid the unfortunate results in question. The first is, not to apply the crushing operation to any stone of a size beyond that which may be termed strictly moderate, a term which it is difficult to define, but which is designed as a caution against regarding lithotrity as desirable for calculi of large size; the second is, not to delay unnecessarily subsequent repetitions of the sitting when the stone has once been attacked by the lithotrite.

These rules are established by practical experience of the operation; but the correctness of the principles enforced is also exemplified by the pathological observations described in the preceding paper. That which has happened to patients who are troubled long after the operation with recurring concretions is, without doubt, a serious injury to the mucous membrane of the bladder, permitting a phosphatic deposit to adhere to some portion of its surface. This deposit increases by aggregation, and is detached in some form as a concretion, which produces symptoms relieved only by its removal. The process is repeated periodically, sometimes with lengthening intervals of time, and with a tendency to cease, if due care be taken, although the term of recovery is often a long one. In other cases, the tendency steadily increases, and the opposite condition follows.

In speaking of injury to the mucous membrane, I by no means imply injury through the use of instruments. Little harm occurs from the modern lithotrite in delicate and careful hands, a remark which does not apply to instruments of early construction. With the latter, injury was often inflicted on the bladder, and strong objections were long entertained to the operation on that account, and very justly so. The causes of injury to the membrane already described as resulting in loss of polish and in roughness, which attracts phosphatic precipitate, are threefold.

First: This morbid change may be caused by the long residence in the bladder of any calculus, particularly one with harsh, uneven surface, and so may have already taken place before the patient seeks relief from the surgeon. In this stage, whatever may be the composition of the stone, the enveloping crust is phosphatic, and the symptoms are severe. Such a condition is no doubt best met by lithotomy, under almost any circumstances.

Secondly: The bladder being healthy, an operator may crush a stone, say of uric acid, but of a size which, although quite within the mechanical power of the lithotrite to crush safely, is still too large to be disposed of in four or even five sittings. There is then some risk that from much contact between the sharp angles of broken stone and the mucous membrane of the bladder, which must take place, abrasions commence, all of which do not heal, and the inner coat is left, at the conclusion of the process, bruised, sore, and slow to recover the natural condition. This might probably have been regained after two or three sittings; but four, five, or six have been more than the membrane could sustain with impunity. With a stone of this size also it is probable that lithotomy would offer equal, if not better, chances of a successful result.

Thirdly: The bladder being healthy at the outset, and the

stone not necessarily being large, but one well adapted for successful treatment by lithotrity, the operator may permit considerable intervals of time to elapse between each sitting. He may do this in the hope of diminishing irritation or inflammation resulting from the previous sitting, desiring not to arouse more disturbance, as he may think, by again applying the lithotrite; and many days may be allowed to elapse in an attempt to combat the existing troubles by rest, medicine, baths, &c. Meantime there is prolonged contact between the rough fragments and the mucous membrane, and damage to the latter is surely taking place. The best remedy in such circumstances is again to crush and so reduce the irritating fragments to fine débris, which, moreover, is largely removed at the same time. I have often seen urine which had been purulent and bloody for days become almost clear within four hours after the use of the lithotrite. At every point of contact between the numerous sharp angles of broken fragments and the delicate lining of the bladder, a minute ulceration commences, and gives issue to a little blood; no sooner are the fragments crushed than the wounded points rapidly heal, and the bleeding ceases. But if the intervals between each crushing are prolonged, dangerous contact is prolonged also, and by repetitions of this error the bladder is brought into a condition in which more or less permanent mischief is sustained, and the phosphatic trouble commences its chronic course. In order to avoid this, then, I repeat that it is essential not to prolong the intervals between each sitting beyond two or three days, unless there is some more important reason for doing so than the presence of cystitis, which is, on the contrary, a ground not for delay but for action. Indeed, no occurrence except repeated attacks of fever or severe orchitis should postpone the use of the lithotrite when the operation has once been commenced.

The next practical question for consideration is the treatment of the bladder itself when phosphatic deposits and concretions are formed there, and show a tendency to remain, or, after expulsion, to be again produced.

The first condition indisputably necessary to success is that the organ, if incapable of emptying itself, should be artificially emptied by the patient in the easiest manner, as often in the twenty-four hours as his comfort demands, and never less than twice a day, however small the quantity left behind. Next, as organs thus affected are by no means always quite emptied, even by the catheter, a small quantity of warm water should be injected once, twice, or thrice daily after catheterism, to wash out the remaining urine if any such there be, and the phosphatic precipitate which will be certainly found therein. For this purpose the four-ounce india-rubber bottle with brass nozzle

and stopcock is the best instrument; one-third only of its contents is to be injected at a time, and this quantity is to run out before the succeeding third is introduced. To the water should be always added either carbolic acid in the proportion of one grain to the ounce, or the solution of permanganate of potash (Condy's) six or eight minims to the ounce. Either of these disinfectant solutions, the first-named being perhaps mostly preferable, should be employed as preliminary to all other injections; they are not in the slightest degree irritant to the bladder, and they deodorise and cleanse the interior. Further, and this is a fact of some importance, carbolic acid does not decompose any solution of metallic salts which it may be desirable to inject immediately afterwards. It ought not to be necessary to add, in passing, that all instruments should be placed, before and after use, in a bath of carbolic acid solution, but double the strength of that mentioned above. This, of course, relates to all instruments which are at any time or for any purpose to be introduced into the urinary passages.

The bladder being thus kept in good sanitary condition, the next consideration is, what agents are to be employed to promote healing action in the diseased mucous membrane? The best are salts of silver, copper, and lead, very weak solutions of which should be used at the first occasion of applying them, watching carefully the result before augmenting their strength, and doing so very gradually. The nitrate of silver should at first not exceed in strength the proportion of one grain to four ounces of distilled water; even one to six ounces is preferable if a patient is more than usually susceptible. It should always be preceded by a cleansing or deodorising injection, to remove from the surface to be acted upon the muco-pus which is coagulated by the solution of silver, and tends to hinder contact with the agent. This injection is to be employed in the gentle manner directed above for the first application. If very little inconvenience follows, a slightly stronger solution should be used after an interval of two or three days, always avoiding an increase in strength sufficient to produce any severe or long-continued pain.

Sulphate of copper should be applied in the same proportion —viz., one grain to six or four ounces of distilled water. An acetate of lead solution of the same strength is a valuable agent, to be used daily, or even twice a day, by the patient himself; but the sulphate of copper, like the nitrate of silver, is to be repeated only every alternate or third day, according to results. It may be remarked here that in the treatment of chronic vesical hemorrhage by astringent injections, such as the solution of matico, or of perchloride of iron, the same rule in relation to the carbolic acid solution, and to the manner of

injecting, should be followed. In the last-named condition, also, the temperature of the injection may be lowered to 40° or 50° F., while, in relation to the subject under consideration, the temperature should not differ greatly from that of the body.

For the removal of small concretions, the eight-ounce elastic bottle, with a large brass nozzle overlapping a No. 10 or 11 gum catheter (described in the Lancet of Jan. 8th, 1876), produces an excellent current, not only inwards but outwards, by expansion of the bottle; and Mr. Clover's aspirator, so useful for dêbris in lithotrity, or for removing last fragments, is equally valuable here. But the object of the injections above described is not to remove deposits from the bladder, but solely for the purpose of acting on the mucous membrane, so as to hinder their formation, and aid in producing a healthy surface, to which they will no longer adhere. By systematically carrying out the plan laid down as soon as they appear, whether after lithotrity or in connexion with chronic disease of the bladder and prostate, the complaint can generally be greatly mitigated, and sometimes it is effectually cured.—*Lancet, Feb.* 2, 1878, *p.* 159.

55.—WHY IS ORGANIC STRICTURE MOST COMMON IN THE BULBOUS PORTION OF THE URETHRA?

By A. Pearce Gould, Esq., M.S., Lecturer on Anatomy at the Westminster Hospital School of Medicine.

The question at the head of this paper appears not to have received the attention it demands, and this is the more remarkable as the answer to it has an important bearing on the prevention of the disease. There is certainly no better established fact than the great relative frequency of stricture in the posterior part of the spongy portion of the urethra. Sir Henry Thompson states that among 270 preparations of stricture which he has examined, the narrowing was situated at the subpubic curvature in 67 per cent., and in the spongy portion of the urethra in front of this, as far forward as 2½in. from the external meatus, in 16 per cent. He adds that behind the junction of the bulbous and membranous portions, "it probably never exists, except from some traumatic cause." Allowing for these few cases, we may therefore conclude that stricture occurs somewhere in the posterior four inches of the spongy urethra in about 80 per cent. of all cases.

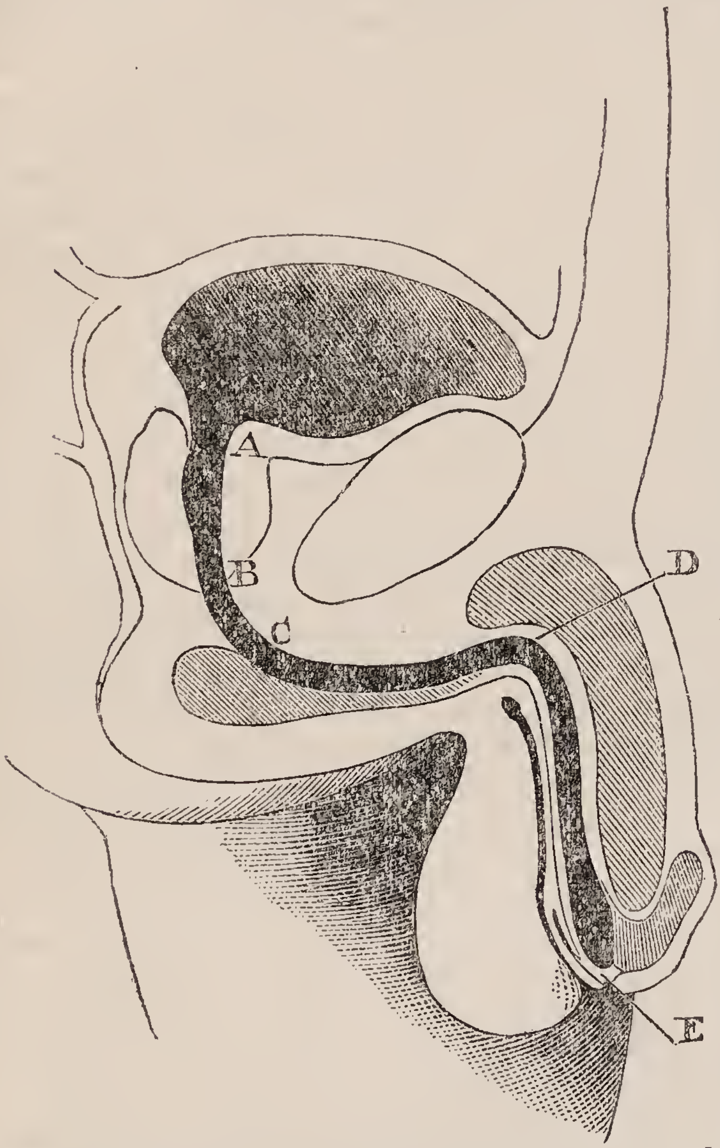
The causes of stricture in this part of the urethra are mainly two—(1) injury and (2) chronic urethritis. Sir H. Thompson gives a table of 205 cases, and if we exclude those due to cicatrisation of chancres and phagedæna, and congenital cases, all of which occur at or close to the external meatus, and are therefore not of interest for the present inquiry, and also three cases of which the accuracy of the observations is questionable,

we find that there are 192 cases left; of these 164 followed gonorrhœa, and the remaining 28 were due to injury. But, on the other hand, Mr. Bryant states that in more than half his cases no definite cause could be obtained. I find that out of the 261 cases of stricture that have been recorded in the University College Hospital Reports, 1871-76, 195 (75 per cent.) followed gonorrhœa, 24 are attributed to injury, leaving only 42 (16 per cent.) to be otherwise accounted for—a result far different from that obtained by Mr. Bryant. I will now answer my original question as to strictures produced by each of these two great causes.

1. *Traumatic Stricture.*—The commonest stricture-producing injuries are falls astride beams or rails, or kicks; these, when severe, crush the urethra immediately over the part struck, or, if less violent, they may push the movable spongy urethra forwards and upwards over the oblique surface of the symphysis pubis, tearing it away from the fixed membranous portion, and rupture the urethra at a slight distance from the point struck, which is nearly always the perineum under the bulb. In rare cases the cause is a punctured wound, and then the membranous urethra may be injured. The urethra may also be lacerated from within by the passage of foreign bodies, as calculi or instruments, in either case at the bulbo-membranous junction, on account of the narrowing of the canal at this situation. For these reasons injuries lead to stricture in the posterior part of the bulb in nearly all cases.

2. *Stricture from Chronic Urethritis.*—Urethritis almost invariably begins at the meatus externus, and spreads backwards towards the bladder, and it is, therefore, more common as well as more severe (acute) in the penile urethra, yet the induration resulting from it is most commonly found at the bulb! This seeming paradox has been thus explained by Sir H. Thompson:—"It is the prolonged existence of subacute inflammation. . . . which is to be regarded as the cause of that deposit, in and beneath the mucous membrane, which, by its subsequent contraction, so commonly produces stricture." M. Guérin explains it by the greater size and vascularity of the corpus spongiosum at the bulb than further forwards, leading to increased plastic effusion. But it is evident that the indurated cicatrix-like tissue of a stricture is the product of a *chronic* and not of an *acute* inflammation, and while greater vascularity and nutritive activity lead to abundant effusion in acute inflammation, they, at the same time, predispose to rapid recovery, and it is the less vascular parts which are more especially prone to chronic indurating inflammations. Inflammation being essentially a lessening of the vitality of a part, a highly nourished tissue will more speedily recover itself. The other and much more frequent cause of chronic inflammation is a *long-continued*

irritation. "In chronic inflammation, on the other hand, the causative force is found to be an irritation, acting not once, but repeatedly; or the irritant is not removed." It would seem that an *unremoved irritant* is the real cause of the chronicity of urethritis at the bulb. A reference to the diagram shows that we may readily divide the urethra into three parts: *superior vertical,* A to C, consisting of the prostatic and membranous portions; *horizontal,* C to D, the bulbous urethra; and *inferior vertical,* D to E, or penile urethra. C D is the seat of chronic urethritis. Now it is evident that any secretion from the lining membrane of A C will trickle down into C D, and that from D E will gravitate out of the urethra; while any secretion formed at or running into C D tends to lodge there, and acts as an *unremoved irritant* to the inflamed surface, and keeps up chronic inflammatory action. Of the fact of this retention of secretion there can be no doubt.



Mr. Hill, in his work on Syphilis and Local Contagious Disorders, says that a very characteristic sign of gleet of the deeper part of the urethra often escapes observation—namely, the appearance of little threads of clotted discharge in the urine. These clots are formed in the deeper parts of the urethra in the intervals of micturition. It is also a common experience that when no discharge appears on compressing the penile urethra, a few drops of glairy mucus escape when pressure is made on the urethra in the perineum; and it is well known that gleety matter comes at the *end* of micturition—*i.e.*, when the ejaculator urinæ contracts spasmodically and empties the bulbous urethra. That the retained secretion is a source of irritation to the

mucous membrane there can, I think, also be no doubt. It is well known that, removed from the urethra and applied elsewhere, it at once excites inflammation, and there is no reason to believe that an inflamed membrane is not irritated by its own product. Balanitis and posthitis often lead to the development of warts on the glans and prepuce, which is attributed to the irritant action of the inflammatory secretion. Retained bronchial secretion is held to be the cause of pneumonia in cases of obstruction of the bronchi. We see in other situations the great advantage of the free escape of inflammatory products (apart altogether from the question of "tension"); for instance, in impetigo capitis, the most important of any local treatment is the removal of the crusts and the prevention of their formation. But a more analogous case is strumous ozæna. In this disease the inflammatory secretion thickens and dries on the surface, and then decomposes, causing the well-known fetor. So long as this continues, constitutional remedies have little or no effect; but add cleanliness to the use of tonics, &c., and remove the secretion as fast as it is formed, and the good effects will be at once apparent. The same principle must guide us in the treatment of urethritis, if we would avoid stricture. We must prevent the retention of irritating discharge, and this can only be done by frequently washing it away. Nature does this for us four or five times daily, but this is not enough; injections should be used, and used frequently. The success attending the use of injections depends principally upon the *frequency* and *efficiency* of their application, and very little upon the nature of the fluid used, provided it is unirritating. Whilst they are regarded merely as local astringent applications they will do but little good; their primary use is to secure *cleanliness*, and they fail entirely in their main purpose unless they free the canal from all secretion. Thus I have found a solution of boracic acid, or of chloride of zinc, one grain to the pint, very efficient, when used often and thoroughly. In the very chronic granular urethritis no doubt the application of a local astringent is useful, but the above remarks refer to the earlier stages of the inflammation. The patient should be directed to raise the organ against the abdomen to get rid of the peno-scrotal bend, and then inject the fluid slowly and steadily, forcing it back quite into the perineum, with his other hand. This should be done as often as possible—six to eight times a day. Frequent micturition should also be encouraged as tending to cleanliness, care being taken to render the urine unirritating by the administration of alkalies. It is interesting to note that stricture is said to occur more frequently in hot than in cold climates. May not this depend upon the less frequent micturition and more concentrated irritating urine in the former?—*Lancet, Dec.* 8, 1878, *p.* 835.

56.—ON THE LOCAL USE OF SOLUTION OF QUININE IN CHRONIC IRRITATION OF THE BLADDER.

By T. W. Nunn, Esq., Surgeon to the Middlesex Hospital.

Some few years since (summer of 1872) I commenced, in the wards of Middlesex Hospital, the local employment of quinine as an antiseptic after operation, in a case of necrosis of the tibia. For the removal of the sequestrum in this case it was necessary to clip away new bone to the extent of some inches from the anterior aspect of the shaft of the tibia. Of course, after the sequestrum, corresponding in length to the bone cut away, was lifted out, a long trough was left, in which the pus accumulated, and became fetid.

My then dresser, Mr. G. Karop, suggested quinine as a bactericide under these circumstances, in answer to my appeal to be furnished with such an agent. I appealed to Mr. Karop, as he had just returned from the Vienna school, and was, moreover, specially qualified, by his histological and microscopical labours, to respond to my request.

The result from the local exhibition of the quinine was most satisfactory. We used a solution of the disulphate of quinine, one grain to the ounce of water, the smallest quantity only of sulphuric acid that would suffice to complete the solution of the alkaloid being added. I have since made frequent use of the solution of quinine as a local application. It appears to me to be especially efficacious, either alone or combined with the bichloride of mercury or the chloride of zinc, in certain forms of soft chancre.

The most striking result, however, is obtained by injecting the solution of quinine into the bladder in those cases where the urine is loaded with pus, and is *intensely offensive;* the bladder being irritable, the desire to urinate recurring every hour, or more often, for example, where the bladder only imperfectly empties itself, or when the continual use of the catheter is called for in enlarged prostate, or in atony of the organ. Within the past few days I have been informed by a patient who has habitually had recourse to the catheter, the urine voided being alkaline and highly offensive, that the injection of the quinine solution has been followed by such an abatement of the sensitiveness of the neck of the bladder that the desire to micturate comes on now only after the elapse of six or seven hours, in place of after the lapse of every hour or hour and a half.

The following is the method of using the quinine as a bladder injection:—Dissolve twenty grains of disulphate of quinine in twenty-five ounces of water by the aid of a few drops of

dilute sulphuric acid or a teaspoonful of *common brown* vinegar. Of this solution inject into the bladder two or three ounces, and let it remain.—*Lancet, Feb.* 23, 1878, *p.* 270.

57.—LOCAL TREATMENT OF SOME BLADDER AFFECTIONS. DESCRIPTION OF A PESSARY-CATHETER.

By REGINALD HARRISON, Esq., Surgeon to the Liverpool Royal Infirmary.

I have recently been using in the local treatment of affections of the bladder soluble pessaries, introduced by means of a special instrument manufactured for me by Messrs. Krohne and Sesemann, and which I have designated a pessary-catheter. From the accompanying drawing it will be seen that the instrument consists of a metallic catheter, open at the end, into which is received a cocoanut-butter pessary (A), containing the requisite drug. After the urine has been allowed to run off, by pressing the stylet the pessary is projected into the bladder, when the instrument is at once removed. The pessaries have been specially prepared for me by Messrs. Symes, of Hardman-street, Liverpool, and contain various agencies, including morphia, opium, bismuth, nitrate of silver, perchloride of iron, and belladonna. The pessaries are so shaped as to form an end for the catheter; and their exposed surface is hardened by a layer of spermaceti, so as to prevent their becoming dissolved in their passage down the urethra. The instrument has been made for me in two sizes; in one the end corresponds with a No. 12 bougie, in the other with No. 8. Pessaries to fit each have been made for me by Messrs. Symes.

In several cases of irritable bladder arising from various causes I have used this instrument with great advantage; in some cases as an adjunct to other local treatment, such as washing out the bladder, catheterism, &c. The treatment of many bladder affections is only to be effectually carried out by local measures, and, in addition to those we are already provided with, I believe the instrument I have now described will be of service. I have certainly

found it so, as it enables the surgeon by one operation, first of all, to empty the bladder, and, secondly, to apply what is required directly to its mucous surface. In this way, I have frequently given a patient a good night by a morphia pessary, where rectum suppositories and other means have failed.—*Lancet, Feb.* 9, 1878, *p.* 197.

58.—ON THE TREATMENT OF ORGANIC STRICTURE OF THE URETHRA, ESPECIALLY BY THE SYSTEMATIC USE OF TAPERING METALLIC DILATORS.

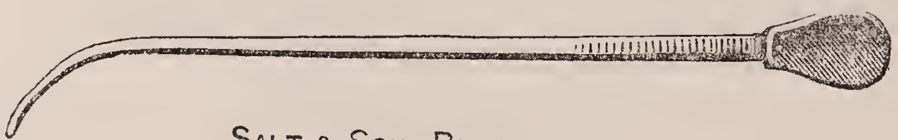
By OLIVER PEMBERTON, Esq., Surgeon to the General Hospital, and Prof. of Surgery in Queen's College, Birmingham.

With very few exceptions—and these I will point out,—all organic strictures admit of benefit by dilatation, and most of them of permanent relief. I do not believe in the word "cure," as applicable to any case of organic stricture by any mode of treatment, and it would be well if the patient understood this a little better than he does in the many instances where elaborate measures have been taken to bring this about, to the exclusion of that instruction of how to be his own surgeon by he himself carrying on the necessary use of dilatation.

There is, moreover, another point that has always struck me in regard to the treatment of permanent or organic stricture, and that is, that it is not needful in all cases to endeavour to restore to the full extent the original calibre of the passage. Given that this, before being diseased, stood at a measure, say of No. 12, that now it is practicable only to No. 4, and by dilatation is brought by the surgeon and kept by the patient at No. 8, to the relief of all and every distress, surely it will be not only easier, but safer, for the latter to keep it at this than to attempt a restoration by a belief in the ability of cicatricial narrowings to be maintained open, without injury to other parts, at the natural standard of the rest of the canal.

In order to carry out this method of dilatation most effectually, a particular kind of instrument is required. We want essentially a "dilator" and a series of "dilators," each instrument to act on the principle of the wedge, and on that of a series of wedges. We want it to be of weight both in handle and body, and to be capable of taking and maintaining the very highest degree of smoothness and polish. Until the last few years I had employed a series of tapered silver catheters, but I found in many instances their construction too light and slender for my purpose, so that I suggested to Messrs. Salt and Sons, Birmingham, the plan on which to make for me a very different set of instruments. These consist, then, of a series of six solid metallic "dilators" of ordinary curve. They are made of the

best mandril wire, and are coated with nickel, which not only preserves them from rust, but imparts a smoothness and almost a greasy feeling, much facilitating their use. They regularly taper from heel to point, varying, in a length of nine inches, three degrees of the English catheter scale. Thus, No. 1 represents a gradual increase of 3 to 6, No. 2 of 4 to 7, and so on to the last, No. 6, 8 to 11. In the engraving two intermediate sizes are shown, viz., 5 to 8 and 6 to 9. I have regarded six as



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a number sufficient in a set, but I need hardly say the ordinary number of twelve may be provided, so long as the same principle in the tapering be carried out, though, as a rule, in practice the numbers below three will hardly be advisable.

The use of conical instruments to effect dilatation in stricture has been common enough in many hands. Brodie used it at the point only. Sir Henry Thompson long advocated the use of "conical steel instruments," and in a clinical lecture published in 1872 he more particularly enforced the advantage of these "conical dilators," as he termed them, but the change in width ended at two inches and a half from the point. What I venture to claim for the instrument I have described is that it is better balanced than others: that it effects dilatation more gradually, and hence more certainly; whilst its influence, extending over the entire length of the urethra, not only secures dilatation at the crucial point, but affects any secondary narrowings also.

As to preliminary treatment. In all cases of long-standing organic stricture, presuming there is no question of retention, which must of course be dealt with by the ordinary rules of surgery, it is desirable that there should be absolute rest in bed for several days previously to commencing dilatation. A warm bath and a purgative daily, as well as the constant fomentation of the perineum by flannels wrung out of warm water, will speedily reduce the hardness in the neighbourhood of the stricture, and will render this, even when cartilaginous, less dense and unyielding.

The "dilator" should be passed, from the first to the last, in the horizontal position, the patient s head being as low as possi-

ble. It should be well oiled, and the urethral opening should be allowed to receive as much oil also as it will retain—the supply in this direction to be renewed when practicable. The first dilator having passed, the further enlargement of the canal may be completed by the introduction of the series either in one sitting or in several, according to the extent of the resistance and the general tolerance of the proceeding on the part of the patient. When rapidly completed, I entertain little doubt that the ultimate resistance is overcome by a distinct spreading and splitting of the fibres of the stricture—something beyond a forcible dilatation of them, and yet not amounting to rupture, for if suitably carried out there is no hemorrhage except a few spots.

As to after-treatment, there is most frequently needed little beyond a fomentation, and rest for a day or two. If pain results, a suppository of ten grains of soap and opium will usually afford relief; whilst rigors, which are infrequent where there is rest beforehand, yield best to the influence of full doses of quinine. Before the patient is discharged he should be taught to pass Nos. 8 and 9, certainly not beyond 10 of the ordinary scale, himself, and be instructed to continue their use each week, or each number of weeks, according to the experience, of which he will be the best judge, of his individual rate of recontraction.

The exceptions to this treatment arrange themselves in two classes: the one where the case is complicated by urinary fistulæ; the other where extensive cartilaginous thickenings mark the severity, as well as the chronic nature, of the obstruction.

For the first, the division of the stricture on a grooved staff, previously passed through it into the bladder — external urethrotomy, as introduced and advocated by the late Professor Syme, is what I believe to be the best plan of treatment.

For the second, something more is wanted than mere dilatation. The cicatricial narrowings will not yield to any satisfactory extent, or if they do their relapse is speedy and certain in the patient's hands. The "something more" in these days, that by many is so strongly advocated, is division by cutting—internal urethrotomy,—the dividing the stricture by the introduction along the canal of a concealed knife, and protruding it, and using it as may be thought needful.

I must own to never having had any confidence in the safety and security of this treatment, and for a very long time I looked with considerable dread on another means that had been proposed for dealing with the stricture—that of forcible rupture; but additional observation and experience has led me to think differently, and rely on this latter method as not only being eminently simple and effectual in practice, but as being almost

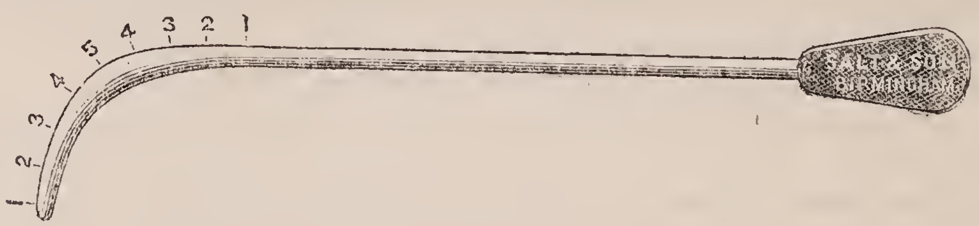
absolutely free from any kind of risk. If, then, the case be one of non-dilatable obstruction, on the plan I have endeavoured to lay down, I content myself with widening the passage sufficiently to admit the introduction of Mr. Holt's instrument.

I will not stay to discuss what may be the exact nature of the tissues making up the inelastic, and probably cicatricial, barrier, but I feel assured that they are adequately split and separated by its use, and that no healthy parts can be injured. I have adopted this treatment in upwards of fifty cases during the last five years, and have not in a single instance observed any symptoms following its application that gave me anxiety; the febrile disturbance, indeed, that followed in some instances being of the very slightest kind. Afterwards the permanent relief was entrusted to the patient, as in the treatment by dilatation only.—*Lancet*, *March* 30, 1878, *p.* 452.

59.—NEW GRADUATED STRICTURE DILATORS.

By T. H. Bartleet, Esq., Surgeon to the General Hospital, Birmingham.

The graduated sounds I have devised, and which have been made for me by Messrs. Salt and Son, Birmingham, are only a modification, but I think an important one, of those described in a recent number of the Lancet by Mr. Oliver Pemberton (see preceding article). The sounds I use are graduated to a scale from the point to the centre of the arc, rising 3 sizes, they are then graduated down again to the size they commenced with at the shaft of the sound. This I consider an advantage, inasmuch as the shaft held in the hand, and which gives instinctively the size of the instrument used, is really the size of the point or



smallest part of the instrument, whilst in many other graduated sounds the point being a No. 2, the shaft is a No. 7, so that while the fingers feel a large sound, the point or part of the sound which may do harm is of the size of a No. 2 or 3; but the graduation down again to the shaft of the instrument renders it in use a bulbous sound, by which you can tell at once when a stricture is passed, a great point in its treatment.

The instruments being nickel plated do not rust, and the surface is rendered beautifully smooth, which I think facilitates the passing.

They are put up in cases of six, and range as follows:—No. 1, from 2 to 6; No. 2, from 3 to 7; No. 3, from 4 to 8; No. 4, from 5 to 9; No. 5, from 6 to 10; and No. 6, from 7 to 11.

I believe sounds have been made in many respects similar to these, but finding no reference to them, and from experience knowing their usefulness, I have felt justified in calling attention to them.—*Communicated.*

60.—ON STRICTURE OF THE URETHRA; WITH SPECIAL REFERENCE TO URETHROTOMY.

By Edward Atkinson, Esq., Surgeon to the Leeds Infirmary; Lecturer on Surgery at the Leeds School of Medicine.

[In this interesting paper Mr. Atkinson confirms our good opinion of Mr. Wheelhouse's method of operation, as performed at Leeds, and which has scarcely received the attention it undoubtedly deserves. He says:]

According to surgical authorities, both English and French, various modes of procedure for the relief of stricture by urethrotomy are recommended; some of which are deemed to preferable in one class of cases and some in another. Distinctions are drawn between the circumstances which render this or that operation the most applicable to a given case. Thus, in old cartilaginous strictures of the membranous portion of the urethra, complicated or uncomplicated by fistulæ in the perinæum, one or another modification of the boutonnière operation is preferred. In cases of retention, where all attempts to pass an instrument through the stricture fail, and the urethra is distended behind the constriction, Mr. Furneaux Jordan counsels the opening of the distended urethra and division of the stricture from behind forwards; while in very tight strictures, where extreme sensibility of the passage resists treatment by dilatation beyond a certain limit, or where such treatment, as soon as discontinued, is followed by a return to the *status quo ante*—and especially when the stricture of this character exists in any part of the straight portion of the canal—internal urethrotomy by Civiale's method, or some modification of it, is often advised.

It is no doubt true of these operations, as it is of many other surgical procedures, that one method proves more frequently successful in the hands of one surgeon, while another method succeeds best in the hands of his friend. This is intelligible enough; for he who has familiarised himself with the steps and the minute details of a given operation, by the habitual adoption of it in a long series of cases (in which numerous varieties of conditions are sure to occur), is more likely to be able to accommodate it to the special requirements of an exceptional case; whereas his occasional adoption of a different method,

with the minutiæ of which he was not so familiar, might not improbably result in disappointment.

At Leeds, it has now for some years been customary to pursue that mode of perineal section—or "boutonnière," as Mr. Teevan prefers to call it—(though why we should go to the French for a name when English serves our purpose I do not exactly understand) which has been fully described by Mr. Wheelhouse; and this, not only in the class of cases referred to above, but in almost every instance where it is considered necessary for the cure of a stricture, whether literally impermeable or not, to incise it, so as to pass a full-sized catheter into the bladder.

The main features of the operation (though they are before the profession) may be briefly epitomised here.

1. The introduction of Wheelhouse's straight button-ended staff, grooved to within half an inch of its extremity, down to the strictured point, with the groove looking forwards.

2. The opening of the urethra *in the groove* and *not upon the point* of the staff, so as to secure at least a quarter of an inch of healthy urethra in front of the stricture.

3. The seizing of the cut edges of the wound in the urethra by two pairs of straight-bladed nibbed forceps, while the staff is turned round and withdrawn sufficiently to hook up the upper angle of the opened urethra. A lozenge-shaped window is thus obtained, through which the stricture can be seen, and often the orifice in it can be detected.

4. The introduction of a finely probe-pointed director into the stricture, and through it on towards the bladder. This, of course, is the *crux* of the operation. It is readily to be conceived that, in cases complicated by false passages, the danger of proceeding to cut on the director, upon the supposition of its having entered the bladder, when it has not, should haunt the mind of the operator; but, as a matter of fact, I think I am justified in quoting Mr. Wheelhouse's expression, that its entrance is "clearly demonstrated by the freedom of its movements." Though difficult cases sometimes occur, I have never seen one in which the director has failed to guide safely.

5. The groove of the director having been turned *downwards*, the stricture is divided on its under surface by the scalpel, and a straight probe-pointed bistoury is afterwards carried along the groove, beyond the external wound, to insure complete division of all obstruction; and then,

Lastly, a most important and essential step, in order to insure the safe conduct of the catheter into the bladder: Teale's probe-gorget is guided along the grooved director into the bladder, dilating the stricture, proving by the gush of urine which flows along it that the viscus is reached, and forming a metallic floor

over which the silver catheter cannot fail to pass safely to its destination.

Of the eight cases successfully operated on by myself, three were old cartilaginous strictures, which would admit of no instrument being passed through the meatus. Two belonged to the category of tight strictures in highly irritable urethræ, which admitted the passage of a No. 4 and No. 6 catheter respectively, but resented further dilatation, which always set up constitutional disturbance. One case was admitted with complete retention; and hot bath, chloroform, opiate enemata, and the evacuation of the bladder by the aspirator, having failed to obtain the introduction of any instrument through the stricture, urethrotomy was resorted to. The last case in which I operated came under none of these heads. He was a man aged 46, who had suffered from spasmodic stricture for five or six years. He was a highly nervous subject and a thorough hypochondriac. Attention to his diet and general health, and the occasional passing of a catheter after a few days' rest in bed, gave only temporary relief. If I attempted to introduce an instrument when going round the wards with my class, I never succeeded; but on returning to him alone half an hour afterwards, I could pass No. 12. Thinking that, by opening the urethra in the perineum and allowing the urine to drain away for a week or two, I should give the irritable urethra a complete rest, and so permanently relieve the spasm, I consulted my colleagues on the case; and, as they all fully concurred in my hopes of the good to be anticipated from such a course, I performed perineal section, freely dividing the membranous portion of the canal throughout its extent, and, having introduced a full sized catheter, withdrew it again, and did not reintroduce it for ten days. This case is still under treatment, but hitherto there seems to be every prospect of a favourable result. Whenever the catheter has been left in in this case, even for four or five hours, a severe urethral rigor, with sudden rise of temperature, has taken place, and that since, as well as before, the operation, sufficiently showing the sensitive condition of the part; and showing also, I think, not only the insufficiency, but the inappropriateness, of treatment in this instance by dilatation alone.

I have now only to refer to the case in which an untoward effect followed the operation, and which was not discovered until six months afterwards.

The patient was a young tradesman, aged 25, who consulted me for a tight and somewhat oblique stricture seated at the junction between the spongy and membranous portions of the canal. No metal instrument would pass, but after many trials I passed a soft *cathetère à boule*, first a No. 3, and at subse-

quent sittings Nos. 4 and 5. Beyond this point I made no advance; and in addition to the unyielding character of the stricture and its obliquity, the urethra was so exquisitely sensitive, that every time the instrument was used, though with the most gentle manipulation and without ever causing bleeding, the patient was in pain for twenty-four hours or more after it. I therefore resolved on dividing the stricture, and, accordingly, performed perineal section, in the manner above detailed, in September, 1875. The bulb was notched, and the anterior half of the membranous portion was divided in the middle line. The stricture was cleanly divided, and the probe-gorget having been used to dilate the posterior portion, a full-sized silver catheter was carried along it into the bladder and retained for thirty hours only. After this, it was introduced every second day for a fortnight, then twice a week for a fortnight more, when he went into the country; but visited me once in a week or ten days to have the instrument passed. The perineal wound closed at the end of three weeks after operation, and he never had a bad symptom nor failed to pass his urine in a good stream. Six months afterwards, he married; and in April 1876, he came to me complaining that he had no satisfaction *in coitu*. The act was completed, but there was no ejaculatory power. The seminal discharge was emitted (or, as he expressed it, "oozed out"), without any sensation of propulsion. I imagined that this was due to recontraction of the canal, but found no difficulty in passing a bougie, and he afterwards passed urine, without assistance, in a good stream. I encouraged him to hope that he would regain the power in time, and recommended him to abstain from sexual intercourse for a while; but several months later he returned with the same story, and more downcast than before, because there was no promise of his marriage becoming fruitful. Now what had happened? Was it the division of the fibres of the accelerator muscle? This would have been the explanation which would probably occur to one at first. But is this muscle the real agent of ejaculation? According to Professor Küss, of Strasburg it is not; or, at any rate, only in a secondary degree. That physiologist says: "At the moment that the sperm is poured into the prostatic portion, this portion of the canal is isolated from the bladder, on account of the turgescence of the *verumontanum*, which is, during erection, in contact with the anterior wall (as shown by Kobelt); and we all know that micturition is impossible during the state of erection. On the other hand, the efferent ducts of the vesiculæ seminales, incorrectly called 'ejaculatory ducts' open *in front of and at each side* of the *verumontanum;* so that the sperm readily passes into the prostatic urethra, which it fills up, but it can go no farther, because at this

moment the urethral sphincter (called Wilson's muscle) contracts and obliterates the membranous portion. The fluid, therefore, accumulates in the straight part of the canal between these two points under high pressure. But the sphincter muscle cannot long maintain its contraction; it relaxes, and immediately the seminal fluid is ejaculated with force due to the sudden relief of the pressure, while the rhythmical character of the emission is due to alternate contraction and relaxation of Wilson's muscle; though doubtless, also, the contact of the seminal fluid with the mucous surface influences the intermittent and tetanic contraction of the urethral sphincter." (Küss.)

It appears to me, therefore, that in the case related above, the loss of ejaculatory power is more probably the result of injury to Wilson's muscle, or the nerves supplying it, either by division or distension, than to injury of the bulb or the bulbo-cavernous muscle, generally called the ejaculator seminis. Whichever be the true explanation, it appears singular that this result should not be more frequently heard of after perineal section.—*British Medical Journal, March* 16, 1878, *p.* 360.

61.—ON INTERNAL URETHROTOMY BY AID OF A NEW URETHROTOME.

By Arthur E. Durham, Esq., F.R.C.S.Eng., Surgeon to Guy's Hospital, &c.

Although my experience of internal urethrotomy is at present limited to but few cases, I may venture, without undue assumption, to assert that my experience in the treatment of stricture of the urethra has been both considerable and extensive: considerable in regard to the number of cases, and extensive in regard to the variety of forms and complications presented, and the methods adopted for giving relief. In common, I believe, with every surgeon of experience, I have become more and more impressed by the want of permanence in the success obtained by the ordinary methods of treatment of stricture of the urethra, however great and striking may have been the relief temporarily afforded; and, from time to time, with increased opportunities, I have met with an increased number of cases in which the ordinary methods have been either altogether inapplicable, or at best have proved ineffectual in affording any satisfactory measure of even temporary relief.

Such being the case, I have often considered the question of internal urethrotomy. But, without doubting or wishing to dispute the excellent results recorded by others, I have been deterred until recently from adopting the operation, because none of the methods with which I was acquainted commended themselves satisfactorily to my judgment. In this I may have

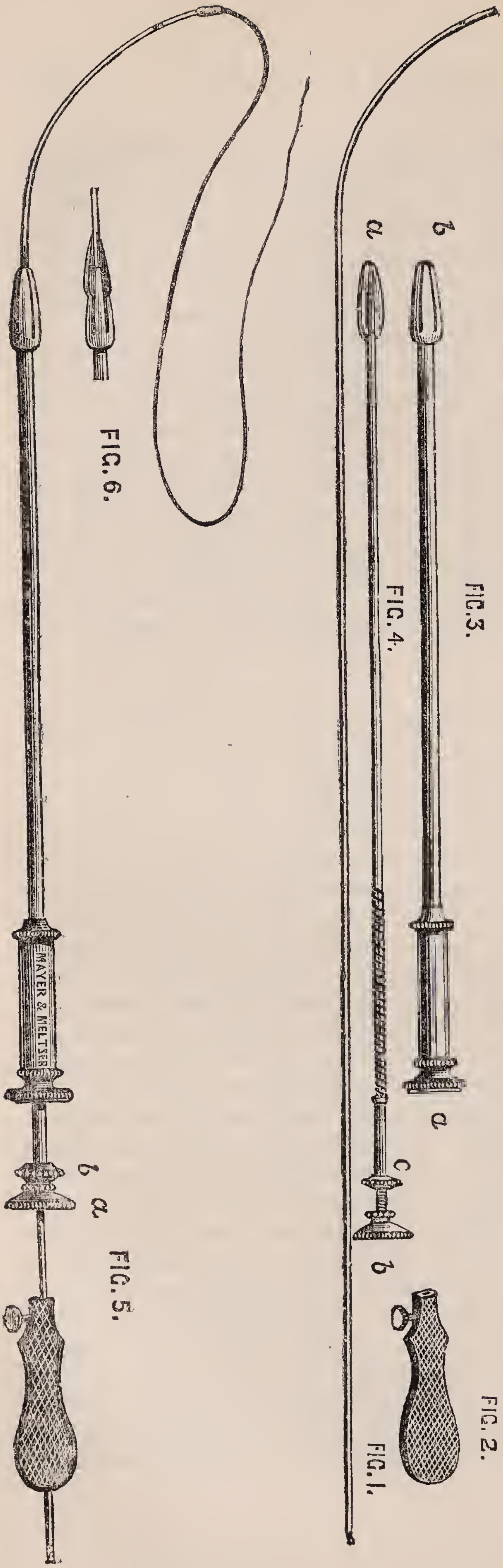


FIG. 1. FIG. 2. FIG. 3. FIG. 4. FIG. 5. FIG. 6.

been mistaken; but it appeared to me that, in many of the successful recorded cases, simpler and less hazardous methods might have proved equally efficacious, and, still more emphatically, that the various instruments devised were either wanting in precision and accuracy of application, or else that they served *to cut a fresh way by the stricture into the urethra beyond rather than effectually to operate upon the strictured part itself.* I have had cases under my care in which internal urethrotomy had been previously practised, but in which I am convinced equally good (if not better and more permanently good) results might have been easily obtained by ordinary methods of dilatation. And I have at the present time under treatment one of the most difficult cases of stricture I have ever had to do with, in which it seems to me manifest that the obvious tortuosity of the existing passage is due to the effect of an internal urethrotomy performed some time since. It seemed to me that what was wanting was some method by which the

natural passage should be restored, and the stricture-substance (if I may use such a term) got rid of by absorption or otherwise, rather than a method by which a new passage should be made above or below the strictured part. With the view of supplying such method, I devised the instrument which I now venture to bring before you. It has gone through several modifications suggested by experience in its use, and I do not for one moment pretend that it has yet reached anything like perfection; but I cannot help believing that the principles involved in its construction and application are worthy, at any rate, of consideration and more extended trial than has yet been made of it.

The instrument consists, in the first place, of a slender steel guide curved in catheter form. (Fig. 1). This is hollow, and either open at the extremity, for adaptation by screw to a filiform whalebone bougie previously passed after the method of Otis and others, or else closed and rounded at the extremity with openings like those of a catheter for the indicative escape of urine. This guide has four longitudinal grooves (too fine to be represented in the figure) throughout the extent of the straight portion. To facilitate the manipulation of the guide, a small ebony handle (Fig. 2) is slided on to it and fixed by a screw. When the guide has been introduced through the stricture and the bladder has been reached, as evidenced by the escape of urine or otherwise, the handle is removed, and the main part of the instrument is slided on. This consists of a tube (Fig. 3) having a hollow cylindrical handle at the proximal extremity *(a)*, and at the distal extremity an elongated slightly tapering olive bulb in which are four slits *(b)*. Sliding within this tube is a second (Fig. 4), somewhat longer, and furnished at the distal extremity with four sharp-pointed sharp-edged blades *(a)*; its proximal extremity is expanded into a circular flat plate *(b)*, between which and the top of the handle of the outer tube is an adjusting screw *(c)*; when this adjusting screw is down on the top of the handle, the blades are securely fixed within the olive bulb of the outer tube, and in this state the instrument is slided down upon the guide until the stricture is reached. The whole instrument put together is shown in Fig. 5, with the filiform conductor attached: *a* is the plate of the inner tube, pressure upon which causes projection of the blades; *b* the adjusting screw. The bulb being then firmly applied to the face of the stricture, the adjusting screw is turned up so far as may be deemed desirable. Pressure upon the top plate of the inner tube will then cause projection of the blades through the slits in the bulb (Fig. 6), and the strictured part of the urethra will be cut to a slight depth at four points in its circumference. Removal of the pressure is followed by

withdrawal of the blades within the bulb by spring action (the spring being in the cylindrical handle of the outer tube). The bulb is then pushed on. If the stricture have been divided to a sufficient extent, the bulb will pass freely on. If it should still be arrested, another and another projection of the blades may be necessary, and can, of course, be easily accomplished. Such projection of the blades is repeated until it is manifest that the strictured part has been sufficiently divided, and the bulb can glide on beyond it. The instrument is then withdrawn and a full-sized catheter at once introduced.

The advantages which this instrument seems to me to offer are these:—

1. The facility and safety with which it may be used, and the accuracy and precision with which it may be made to act upon the strictured part of the urethra and no other.

2. The exactly limited depth to which the incisions extend. They can scarcely be made to a greater depth than what corresponds to the proper circumferential extent of the urethra.

3. Four incisions of slight depth into the stricture-substance (if I may use such a term as I have said), are made instead of one incision of comparatively great depth, as with the ordinary instruments, and which may, and probably often does, extend considerably beyond the stricture-substance into healthy tissue. Thus the stricture-substance is more effectually treated, and the healthy tissues have better chance of escape. And here I would point out that the effect of incising the cartilaginously hard stricture-substance may be not inaccurately compared to the effect familiar to all surgeons produced by notching or incising the thick callous edges of certain old external ulcers. In this latter case, we see how speedily new action is set up, and how the indolent indurating material disappears and a healthy healing process is started and established. May it not probably be the same with the stricture-substance?

4. The stretching and dilatation of the previously strictured part is obviously rendered more uniform.

5. And, lastly, the healing or cicatrising process is facilitated, inasmuch as the extent of cut margin from which cicatrisation can go on is necessarily greater in the case of four shallow cuts than it would be in the case of one cut made so deep as to allow of equal dilatation of the passage.

Having thus described the instrument I have devised, and indicated what appear to me to be its advantages, I pass on to state the results of my experience of its use; and, very limited although such experience has at present been, the results have been such as to encourage me to persevere in its use in any seemingly appropriate cases that may come before me. Hitherto I have only had what seemed to me legitimate occasion for

resorting to this method of treatment in twelve cases, but in all of these the results have been so far most satisfactory. To two of these cases I would especially ask attention, inasmuch as in them a degree of success was easily and speedily obtained far beyond anything that could have been expected to have been attained with equal ease and speed by any other method with which I am acquainted.

In none of the cases was there any hemorrhage worthy of note. I have often seen more follow attempts at catheterism. In one case only was there anything like severe constitutional disturbance; but the patient in this case was a man of very intemperate habits; the symptoms from which he suffered more resembled those of a slight attack of delirium tremens than those of traumatic delirium; he had but one rigor, and his temperature was only over 100 deg. on one day. In one case only was severe pain complained of after the operation; but in this case I am inclined to believe that the pain was due to the fact that I had introduced and left in a catheter of too large size (No. 12). In no instance was there any indication of extravasation of urine or of perineal abscess. In all cases, when last seen, the patients were able to pass urine in full stream, and a large sized catheter (10 or 12) could be introduced without hitch or hindrance. In all instances, the progress was more rapid, and the satisfactory result was obtained with less confinement to bed than, so far as I can judge, would have been the case if the ordinary treatment by gradual dilatation had been adopted, even supposing that such treatment had been applicable. In all cases except one, a catheter changed from time to time was fixed in for some days after the operation, the number of days varying according to the conditions and indications of the case. In the one exceptional case alluded to, the patient himself pulled the catheter out during the night after the operation. I thought the opportunity a good one for testing the desirability of dispensing with the retained catheter, and did not replace it; but the result of the experiment was not such as to encourage me voluntarily to repeat it. The first day, the patient passed urine in an excellent stream; but afterwards the stream became very much smaller, and there was considerable pain during micturition. It was not until a week afterwards that a No. 10 could be again introduced with facility, smaller instruments having been passed in the meantime, but not fastened in.

The questions, as to whether a catheter should be fastened in after the operation, and, if so, how long it should be retained, appear to me to be very important, in regard both to the rate of progress and to the ultimate result of the case. I am inclined to think that the catheter should, unless contraindicated, be

retained until the granulating process has been fairly established on the cut surfaces. But the only safe rule appears to me to be that in this, as in other points in practice, we must always be guided by the conditions and indications presented by the particular case with which we have to do, regardless of any hard and fast rule, however well founded, according to general or average experience, such rule may appear to be.

I may be permitted to give a few details of the two cases to which I have referred, as affording, in my experience, the best illustrations of the success and value of the operation.

The first case is that of a gentleman, thirty-eight years of age, who had suffered very severely from stricture of the urethra during the long period of nearly twenty years. He had had several attacks of retention, relieved from time to time by opium and hot baths. During the years 1867-1868, he was treated by constantly repeated applications of potassa fusa. After submitting to this treatment for two years, and, to use his own words, "enduring the most agonising pain with fortitude, he considered he had been sufficiently patient and persevering." No larger sized instrument than a No. 2 could ever be got into the bladder. He gave up his case as hopeless, and went to reside in Jersey. While there, his sufferings were somewhat diminished. He returned to London in January, 1870, and again submitted himself to the potassa fusa treatment; and, although he experienced no benefit whatever and suffered excruciatingly, he continued some months under this treatment, and then gave it up in despair. From May, 1870, to May, 1876, he received no surgical treatment whatever, the frequent attacks of retention being relieved by opiates, warm-baths, and suppositories. He became altogether "incapacitated from attending to business, could not enter society, and indeed was unable to venture many yards from home, in consequence of the bladder being unable to retain more than an infinitesimal quantity of urine." "More than fifteen minutes' sleep at a time could not be taken; and by May, 1876, his general health was completely broken down, and his condition was such that neither he nor his friends believed he could survive much longer." Under such circumstances, I saw him; and his condition was indeed deplorable. Any attempt to touch the urethra with an instrument gave rise to the most excruciating pain; and it was obvious that, even supposing a fine instrument were introduced under chloroform, it would be impossible to retain it so as to effect any good. The bladder was in a most wretched condition. The only thing to be done appeared to me to afford relief by means of opening the urethra behind the stricture, according to the method of Mr. Cock. This I did with most satisfactory result. His sufferings were at an end. He slept excellently well the whole of

the night after the operation. The bladder, which before more resembled a sloughy abscess-cavity than anything else, gradually regained a healthy condition, and he steadily progressed, gaining health and strength until, free from all pain, he was able to get about, comparatively strong and well, with ease and comfort. He passed his urine freely through the perineal opening either with or without the aid of a catheter. I have mentioned these details to show the severity of the case with which I had to do. For some months, he was so well satisfied with his condition, that he declined to have any attempt made to restore the natural passage. In January of the present year, he wished, however, to have an attempt made; and, accordingly, chloroform having been administered, I performed internal urethrotomy in the manner I have described. There was some considerable difficulty in getting the guide into the bladder; but the long rest the urethra had enjoyed had permitted, at any rate, some of the inflammatory swelling to subside, and I was able to succeed. By alternately projecting the blades of the instrument and pushing on the bulb, I at last succeeded in tunnelling through all the strictured part of the urethra. The cutting must have extended through a length of at least two inches and a half. A No. 10 catheter was at once passed and fastened in. There was very little bleeding. From the day of the operation, the patient never suffered any pain worth speaking of, nor had he any bad symptom of any kind. The passage was perseveringly kept open by catheters, retained at first, and afterwards introduced whenever he wanted to pass urine, for some time; and the perineal opening was allowed to close. Now, seven months after the operation, he passes urine in a good full stream, and a No. 10 catheter slips along the passage without hitch or hindrance. The urine is clear, and he has returned to active business occupation. I know no other method by which a like result could have been expected to have been attained.

The other case is that of a patient, thirty-six years of age, who had been the subject of stricture, gradually increasing in severity during ten or twelve years. He had led a very hard life in India and South America, constantly in the saddle, exposed to all sorts of hardships, and never able to get proper surgical treatment. When he came under my care, in March last, his perineum and scrotum were riddled with sinuses, through which the urine escaped, only a few drops coming from the orifice of the urethra. He was emaciated and broken down in health, and was evidently suffering most severely. After several attempts, I passed a No. 1 elastic catheter into the bladder, and, two days afterwards, performed internal urethrotomy In this case, I had to cut through three distinct strictured por-

tions instead of one continuous one. A No. 10 catheter was passed and fastened in. The sinuses were laid open to such extent and in such manner as seemed desirable. No bad symptom followed; but the greatest relief was experienced. In three weeks, the patient was up and about. The sinuses had apparently almost filled up. He was not allowed, however, to pass his urine without the aid of the catheter for another fortnight or three weeks, at the end of which time the old sinuses appeared fairly closed up. The improvement in general health and condition was rapid and marked. Three months after the operation, he declared himself better and stronger than he had been for years, and altogether free from pain and trouble about his urinary organs. He continued, however, to use the catheter at least once daily.

How far the good results obtained in these, and the other cases to which I have more briefly alluded, will prove permanent, remains to be seen. But, at any rate, a large measure of immediate relief has been afforded. And I cannot help believing that the new cicatricial tissue of an incised surface may prove less likely to contract speedily than the tissues around a stricture after ordinary methods of dilatation, and to become the seat of fresh inflammatory swelling and thickening.

In conclusion, I would venture to say that, although I have spoken so far in favour of the operation of internal urethrotomy, and so hopefully as to the results to be expected from it, I should be very sorry to leave the impression that I would advocate or practise this operation indiscriminately in all cases of stricture. For ordinary cases, I believe the ordinary method safest and best. Internal urethrotomy must be regarded as an exceptional operation, to be practised in exceptional cases. But, as such, I believe it well worthy of adoption—at any rate, of more extensive trial than it has yet had in this country.—*British Medical Journal, March* 16, 1878, *p.* 358.

62.—ON INTERNAL URETHROTOMY.

By W. F. Teevan, Esq., B.A., F.R.C.S., Surgeon to the West London Hospital; Surgeon to St. Peter's Hospital, &c.

Within the past few years, the operation of internal urethrotomy has occupied much attention in this country; the principles of the procedure have been more clearly established and better carried out, great improvements have been effected in the instruments used, and I think it may be safely predicted that internal urethrotomy will, in the future, attain that position to which it is justly entitled.

I would firstly remark that I consider most urethral strictures are best treated by gradual dilatation, carried out by means of

soft instruments; that an operation is but seldom called for, and ought, as a rule, to be only resorted to after milder measures have failed. I would also observe that, pathologically, there is no evidence to prove that a stricture can be cured; but practically all strictures are curable, provided an instrument is occasionally passed at regular intervals for life.

Now, if an operation be indicated, what are the requirements it must fulfil? what, in fact, is demanded of it to prove successful? I do not think the answer can be found in any English work; we must ask French or American surgeons. The former would reply that the insertion of the "*piece d'allongement*" is required; whilst the latter would say that a "cicatricial splice" is wanted. The above expressions convey, in a very clear and concise manner, what is indicated. We have to enlarge the contracted urethra by letting into it a splice of new tissue, which is of a necessity cicatricial. We know that cicatrices are endowed with varying powers of contraction; those, for instance, which result from the clean cut of a surgeon's knife shrink but little, whereas those following lacerations contract greatly. Hence, therefore, a cicatrix made by a clean incision possesses the minimum amount of contraction, whilst that following a laceration has the maximum degree; and, inasmuch as we want a cicatrix which will contract as little as possible, we must choose a cutting operation, and not a tearing one, like the so-called "immediate dilatation."

Let us not forget these two following important surgical facts, which we constantly see, and which vividly show the relative results of cutting and laceration. When a surgeon incises the healthy urethra of a man, in the operation of lithotomy, no stricture follows when the wound is healed; but if the same man had been kicked in the perineum and his urethra torn, a stricture of the worst description would have ensued. Hence our choice is limited to cutting operations. Now, there are three forms of urethrotomy—external, subcutaneous, and internal. The external I regard as a severe procedure, which ought to be very rarely required; the subcutaneous is only adapted for cases where there is but a single stricture; whilst internal division can deal with any number of strictures; and, as it is a procedure which is attended with but little risk to life, it must be regarded as our stock operation for stricture. In what cases would I operate? If the stricture be non-dilatable, or, if dilatable, it contract again as fast as it is stretched; or if there were numerous fistulæ combined with a tough stricture; or if the process of gradual dilatation were attended with great pain or constitutional disturbance; or, lastly, if continuous dilatation had failed. It may also be premised that penile strictures and those of traumatic origin

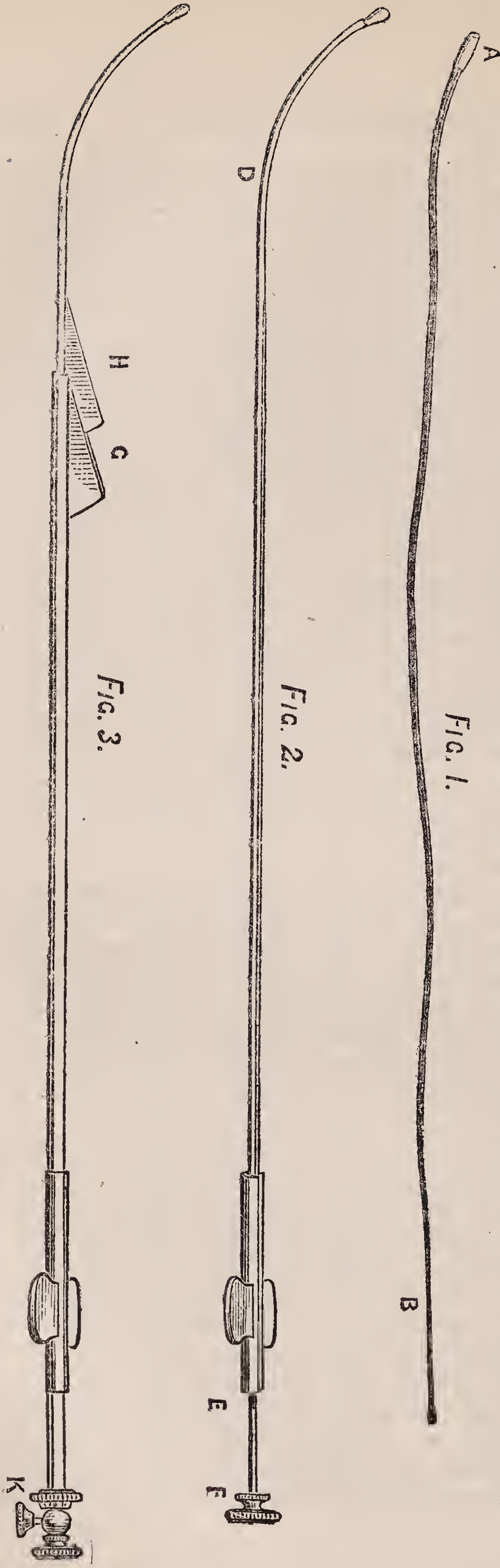


Fig. 1. Fig. 2. Fig. 3.

are not usually amenable to dilatation, and require to be cut. Having determined on internal urethrotomy, shall we divide the stricture from before backwards, or from behind forwards? The answer to this question has divided surgeons into opposite camps, and acrimonious discussions have taken place. As both parties have obtained good results, they have each attributed them to the particular method they have employed. I would venture to say that, in the hands of a skilful operator, equally good results will follow either method, but that the division from behind forwards requires a great deal more skill and care than the other, in order to determine the length of the incision, for it is not always so easy to judge where it shall end. The cutting operations may be of two kinds. Firstly, scarification. In this operation, a number of notches or small cuts are made into the stricture, but not through it, for they are not extensive enough for that. The instrument which makes the cuts is called a scarificator, and usually has two, three, or even four small blades. Secondly, internal ure-

throtomy, in which the stricture is completely divided at one cut by an instrument named an urethrotome, which generally has but one blade. There is a very great difference in the results of the two operations. The scarificator merely notches the stricture sufficiently to allow itself to pass through, whereas the blade of the urethrotome cuts the stricture in two, and permits of the passage of a vastly larger instrument than itself, for the simple reason that, the stricture having been completely divided, there is no longer any resistance. At one time, scarification was much employed in France; but it has, I think, been almost completely abandoned, as the results obtained by it were of a very fleeting character. It has been almost unanimously and emphatically laid down by French and American surgeons that, to obtain a good result, a stricture must either be torn through or cut through; and, as the former operation does not fulfil the requirements I have alluded to, it only remains for us to cut completely through the contraction with the urethrotome.

An enormous number of urethrotomes have been invented, and many of them have, I think, earned the late Professor Syme's condemnation, that they were "terrible engines of war." Until a few years ago, Civiale's urethrotome was, perhaps, more used than any other for dividing from behind forwards, and Maisonneuve's for cutting from before backwards: the latter instrument has been considerably improved. A good urethrotome ought to fulfil the following indications. 1. It should, when introduced, declare with certainty whether it be in the bladder or not. No urethrotome ought to be used which does not do this, for much discredit has been unjustly brought on internal urethrotomy by surgeons employing instruments which did not prove where they had gone to. Hence, false passages, and even the rectum, have been divided instead of the stricture. 2. The knife should not wound the healthy urethra. 3. The staff of the urethrotome should be very slender, so that it can be passed through very tight, narrow, non-dilatable strictures. 4. The instrument should not only tell where the incision is to begin, but where it is to end.

Now I believe that the urethrotome, as modified and improved by Sédillot, Gouley, and myself, fulfils all the above requirements. In Maisonneuve's instrument, the groove in the staff extended through its entire length, so that the knife went into the bladder, which was unnecessary; and, as the slit was usually blocked up by blood or mucus, and hence the withdrawal of the urine, which formed no part of the operation, could not be effected. The groove in my staff is filled with a closely fitting stylet (E F, Fig. 2), so that, when the wire is taken out, urine will flow if the instrument be really in the bladder. Maisonneuve thought he had protected the healthy urethra from possible injury when he

put a metal knob or button on the apex of his triangular blade. *Post mortem* examinations made in Paris and New York demonstrated that with his knife the healthy urethra might be cut in numerous places undesignedly. In one instance, the mucous membrane of the canal was cut in its entire longitudinal axis. Then, again, this knob on the blade pushed away some of the outlying fibres of the stricture, and so prevented their division. I have removed the button on the blade, and protected the urethra from injury by encasing the knife, H (see Fig. 3) in a double sheath, G. The peculiarity of my double sheath is, that it runs outside the staff in telescopic fashion, and not inside it, as in the urethrotomes of Sédillot and Gouley. By this modification, the calibre of the staff is not increased in bulk, whereas it is greatly strengthened by the stem attached to the sheath embracing it. The sheath also represents the surgeon's finger in the urethra; for it feels for the obstruction, tells him where it begins; and. when he thinks he has divided the stricture, it assures him of the fact or otherwise. Lastly, it makes the parts tense for us when we stretch the penis forcibly upwards towards the handle of the urethrotome. I have had the end of the staff of the urethrotome tunnelled like Dr. Gouley's, so that the instrument can either be slipped over a fine filiform bougie introduced into the bladder, or it can be screwed on to the "bougie conductrice," A B (see Fig. 1), and made to follow it into the bladder. The groove in my staff stops at D (see Fig. 2), two inches from the end; so that, when the urethrotome is in the bladder, the curved non-grooved extremity is in the prostate and bladder. Inasmuch as there is never any stricture in the prostate, there is no object in carrying the groove to the end of the staff, as it is in the urethrotomes of Sédillot, Gouley, and Maisonneuve. The conducting bougie A B, Fig. 1, is of great value; for in some cases it is impossible to introduce the urethrotome, not because the stricture is so tight, but because the passage is so tortuous. If, however, the bougie be first passed, and the staff of the urethrotome be screwed on to it, it will follow the former into the bladder. This procedure must be conducted very slowly, otherwise the bougie may be doubled up if the urethrotome be pushed too quickly after it. When the instrument is apparently in the bladder, I withdraw the stylet E F (see Fig. 2) to verify its position. If urine flow, I operate; if it do not, I withdraw the urethrotome and try again another day. Supposing, however, that the urine escapes, I stand on the right of the patient, and take the knife enclosed in its double sheath, and, protruding the former a little, I insert it into the groove of the staff, and immediately slip the double sheath *over* and *outside* the staff. I then withdraw the knife within the sheath, and, clutching hold of the penis just behind

the glans, I draw it forcibly towards the handle of the urethrotome, which is held by an assistant standing opposite me; and with the right hand push the stem of the sheathed knife slowly down the urethra till it arrives at the obstruction, against which I keep it steadily pressed. By these two manœuvres, I have ensured that the stricture is made perfectly tense, so that it can be cleanly and completely divided. I now protrude the knife for half an inch, knowing that that is the minimum cut required to divide even a ring-stricture not thicker than a thread. The knife is then withdrawn into its sheath, which is pushed forwards to see if the stricture be completely divided or not. If it be not, the process is repeated, each cut being half-an-inch long, till everything is cut. I generally divide the strictured urethra in the roof, as I think that is the best situation, for the bulb is thereby avoided. If the surgeon prefer, however, to cut the floor of the canal, all he has to do, when he has passed the staff of the urethrotome, is to reverse the instrument, so that its point is turned behind the prostate, as in lithotrity. The urethrotome can be made with a lateral or inferior blade, if desired. As soon as I have ascertained that the canal is perfectly free from one end to the other, I withdraw the instrument and introduce a No. 25 silver catheter, for the purpose of demonstrating that the urethra has been restored to its normal calibre and to completely empty the patient's bladder. By ensuring that the bladder is empty, the patient can go for some four or six hours without wanting to make water, by which time the wound will be covered with a firm clot, and the pain in micturition be considerably diminished. I do not leave any catheter in the bladder after the operation, and I allow the patient to pass his urine naturally. I believe I was the first surgeon in this country to dispense with the use of the catheter after the operation. If there be one practice more persistently insisted on than another in English surgical works, it is that of the necessity of the use of the catheter after urethrotomy, the instrument to be either left in the bladder or else employed to draw off the patient's urine; and the writers point out the disastrous consequences which will take place, in the shape of abscess, fistula, or infiltration of urine, if the practice be not observed. To Dr. Gouley of New York belongs the credit of having shown the utter groundlessness of the surgeon's fears.

After the operation, I am in no hurry to commence the passing of instruments, usually waiting till the fourth day, and not introducing them oftener than twice a week. At the end of ten days, I begin to teach the patient how to pass a catheter for himself, and order him to do so every Saturday night till further orders. By passing a large No. 25 bougie or catheter

several times after the operation, the insertion of a good "cicatricial splice" is guaranteed.

Now as to results. I have operated in all on thirty-three cases, all of them of the worst description, and for that reason relegated to the operation, without a single death. In one instance, the urethra was so indurated in its entire length that I left in a catheter after the operation, to set up urethritis, and so lessen the thickening. The inflammation was, however, more than I desired; and abscess, followed by fistula, ensued. The case ultimately got quite well. In one case only had I troublesome bleeding. It proceeded from the meatus, which I had divided, and was a hint to me for the future not to cut the meatus at the same time as the stricture, but several days before. As a rule, not more than about a dessert-spoonful of blood escapes either at or after the operation. Secondary hemorrhage I have never seen. Rigors occurred in about two-thirds of my cases. I look upon them as entirely nervous and of no importance. It is stated in books that rigors are due to the passage of the urine over the wound. This, however, cannot be the correct explanation; for, unless pyæmic, they are never seen after lithotomy or external urethrotomy. I look upon rigors after internal urethrotomy as caused by the sudden *stretching* of the nerves in the wound through the distension of the canal by the urine. In three of my earlier cases, I had to repeat the operation, as my incisions had not been sufficiently free. I am sure that every one who performs the operation will be pleased with the soft supple cicatrix which follows it, so different from the rough, tough, irregular cicatrix which forms the so-called "immediate treatment," which is neither more nor less than absolute laceration, not always of the tough stricture, but sometimes of the unoffending healthy urethra, where, as elsewhere, "the weaker goes to the wall."

Now for statistics. I think they will be found to be eminently satisfactory, and will carry conviction. They are the largest, I believe, which have ever been placed before the profession, and show what internal urethrotomy can accomplish. No other operation for stricture with which I am acquainted can produce such favourable results. I consider that no operation can be performed on the urethra without a certain amount of risk; but how slight that risk is you will immediately see. I find by examination that the operation has been performed by six surgeons in London, Paris, Mobile, and New York, one thousand and ninety-five times, with but ten deaths; and there would probably have been two deaths less had it not been for the crowded state of the wards in the Neckar Hospital during the Commune.

In conclusion, let me say that the deductions drawn

from the experience of a solitary surgeon may be misleading but that the conclusions which must be arrived at from the overwhelming mass of figures which I have placed before you loudly proclaim that the operation of internal urethrotomy claims your consideration and demands your support.—*British Medical Journal, March* 16, 1878, *p.* 361.

AFFECTIONS OF THE EYE AND EAR.

63.—ON THE TREATMENT OF INTOLERANCE OF LIGHT.

By Bowater J. Vernon, Esq., Ophthalmic Surgeon to St. Bartholomew's Hospital.

As regards the treatment of these cases, no very precise rules can be laid down. Whether or not the disease can be justly ascribed to any peculiarity of constitution—formerly called scrofulous, there is no doubt that the more urgent symptoms will often yield when some source of irritation elsewhere than in the eyes has been discovered and removed. So frequently is an attack connected with errors of diet, that the disease has been called by men of experience "gastric ophthalmia;" and if we reject the term itself, it will nevertheless remind us how necessary it is to watch very carefully the diet and the general regime of nurseries which are generally supposed to be well regulated; for although the disease is most frequently met with amongst hospital patients, it is no less true that the pampered and over-fed children of well-to-do persons occasionally suffer most severely.

Foremost amongst local remedies, and one which should be firmly persevered with, stands belladonna in some shape or form. The extract dissolved in boiling water makes a most soothing fomentation, but it sometimes happens that the incessant warmth and moisture which attend its use will give rise to great cutaneous irritability, and to the efflorescence of herpetic vesicles upon the parts around the eyes. On the other hand, the solutions of atropine do not always give satisfactory results. They have more than once been known to cause dangerous symptoms of poisoning; but the objection to them is rather of another kind, that they are uncertain and inert. The gush of tears which follows their application has something to do with this. At other times these solutions are found to be exceedingly irritating, for their composition is unstable. Under such circumstances, it may be possible to gain the soothing effect by the use of a liniment in which the extract has been rubbed up with glycerine. In any case, it will be found that, as soon as the full effect can be produced upon the pupil, the most distressing of the symptoms will subside. On the whole, it will be found

extremely difficult to produce the full effect of belladonna upon the eyes of out-patients without considerable delay and without great waste of material; and I think that it will be found more satisfactory to see the solution properly applied twice a week, than to trust it to inattentive hands for more frequent use. Another remedy which I have found of very great value in numberless cases is the cold-water douche to the nape of the neck. One or two applications in the morning will sometimes dispel the intolerance as by a charm. This plan, however, can hardly be properly carried out except in a hospital, or under the favourable conditions of private practice. When the acute stage has passed, and the cornea is left roughened, and with its surface faceted with the scars of minute vesicles, the local application of calomel or of the yellow mercurial ointment will once more be found of great service. An objection to this ointment is that it decomposes, changes its colour, and becomes irritating, so that it should be used when freshly made or not at all. Again, neither of these remedies should be employed when there is much intolerance, nor when there is ulceration of the cornea. Their effect is probably in great measure mechanical, and when used under suitable circumstances, their value is very great; but their indiscriminate use in affections of the cornea has sometimes brought ridicule upon the medical adviser.

The ordinary and very various plans of general treatment which have been found useful are familiar to everybody, but I would put upon record, that in the summer months, when the disease of the eye has been supplemented with a turgid mucous membrane within the nose, and with thickening of the upper lip, I have often found chlorate of potash in large doses a most useful medicine.

Although it sometimes happens that a child may pass through an attack as above described, and may escape with his eyes unhurt, it more often happens that one or both of them will be permanently injured, to the extent of one or more small maculæ, no bigger than a pin's head perhaps, but, situated, as they generally are, in the very centre of the cornea, over the pupil, they are most detrimental to good sight. No doubt the impairment of sight which such an opacity will cause depends much upon the circumstances of the patient, and to some extent upon his individual disposition; for in the case of nervous and sensitive patients, these faint opacities not only prevent accurate vision, but they cause great annoyance by the distortion and multiplication of small objects. In some instances relief may be found in the employment of weak concave glasses, but, as a rule, the child soon learns to suppress the image of the faulty eye, and thus deprives himself of the advantages of binocular vision.

A more immediate, and a very disheartening result, is the extreme tendency to recurrence which these cases often exhibit. In spite of every care on the part of the medical attendant, the slightest irregularity of health or habit is sure to be the signal for the renewal of the intolerance and its attendant spasm of the eyelids. The attacks frequently recur at stated times—in the spring and autumn, for example; they appear as the result of any kind of external irritation, however trivial, such as exposure to wind and sunlight; the intolerance is again extreme, with copious lachrymation. The ciliary vessels may appear enlarged, and there may be a circumcorneal zone of congestion; but beyond a slight haziness around the original seat of disease, there is no other change visible. These recurrences are not usually very severe, but they take place almost without warning, and the disease may as suddenly disappear. They interfere sadly with education, and are exceedingly difficult to treat, inasmuch as the surgeon has to overcome a tendency to disease rather than disease itself. As this habit of recurrence will, under favourable conditions, wear itself out by degrees, it is well to do what we can to ward off all possible sources of irritation. In attempting this, much help may be derived from some form of counter-irritation. The application of iodine paint over the brow or behind the ear is often of great service, and still better results have been obtained by the insertion of a small seton, a single silk thread behind the ear; this causes but little inconvenience, and when concealed by the hair, can be worn without interfering with school-life.

Another unfortunate result of the formation of small corneal opacities is the occurrence of strabismus. The exact mode by which this is brought about is not very clear, and probably more than one factor is concerned. The explanation commonly given is, that in consequence of the opacity of the cornea, the distinctness of the retinal images of the affected eye is more or less impaired, and the difference in the clearness and intensity of the retinal images of the two eyes is often very confusing and annoying to the patient; and in order to escape from this annoyance, he involuntarily squints with the affected eye, so that the rays from the object may impinge upon a more peripheral, and therefore less sensitive, portion of the retina, and the image of this eye be consequently so much weakened in intensity, as not to prove any longer an annoyance. The direction in which the deviation may take place is generally determined by the strength of the muscles, and is almost invariably inwards, or inwards and upwards. The image of the squinting eye will be gradually suppressed, and then amblyopia from disease of the eye will be superadded to the weakness of sight caused by the original affection. It has been

pointed out by Pagenstecher that hypermetropia exists in very many of these cases, and must be regarded as the true cause of the strabismus; while other authorities, and especially Donders, think that the inflammation which causes the corneal opacity may extend to some of the muscles, and at first bring on a spasmodic, and then an organic, contraction of the muscular tissue.

Such squints as these are not favourable cases for operation, unless it be for the sake of some improvement in personal appearance.

Intolerance of light is also occasionally a symptom of great importance, pointing to the existence of what may be termed sympathetic irritation, arising in one eye as the result of some injury previously inflicted upon the fellow-eye. It is one of the earliest symptoms of this condition, and if the warning be attended to, and the injured eye be at once removed, the vision of the other and sound eye may be preserved intact. Clinical observation shows more and more clearly that many cases of sympathetic inflammation are preceded by a premonitory stage, which may be called a stage of irritation only, where as yet there is no structural lesion. Most certainly all cases are not preceded in this way, and this period of incubation, so to speak, is not as a rule a very lengthy one, although Mr. Carter has put on record some observations which show that sympathetic irritation may occasionally exist for a very considerable time, and with no development of destructive inflammation.

Hysterical asthenopia.—The anomalous and wide range of symptoms which are now included in the term "asthenopia" formerly admitted of no very precise explanation, and we consequently find them arranged under very different headings in the writings of the older ophthalmic surgeons. The announcement by Donders that very many of them were to be explained by the difficulty in accommodating for near objects and for prolonged work by eyes which were naturally deficient in refractive power (hypermetropia), was shown to be so well founded that it led to a complete reaction of opinion, during which there was some danger of its being thought that nearly all functional defects of the eye were directly or indirectly the result of errors of refraction, and were to be cured by the use of proper glasses. There is abundant evidence to show, if necessary, that many a young man's education and career have been checked, if not entirely spoiled, because the difficulty with which he pursued his studies admitted of no rational explanation, and consequently of no relief. On the other hand, anybody who has any great experience of the out-patient rooms of a large hospital will soon come to the opinion that there is a distinct set of individuals whose symptoms are those of aggravated asthenopia,

and yet in whose eyes the error in refraction is so trifling, that it is impossible to suppose that it can be the sole, or even the chief, cause of so much inconvenience. Such cases are for the most part young women. To the ordinary symptoms of asthenopia are added those of hysteria. Intolerance of light is nearly always present, and this is sometimes aggravated by the habit of wearing dark glasses, especially amongst the better class of patients. Although these cases bear a general resemblance to the ordinary form of accommodative asthenopia, yet there are distinctive features between them. In the asthenopia of hypermetropes, there is an undoubted error in refraction, and the pain and sense of fatigue complained of can be referred to the efforts to accommodate; and they are increased or diminished in proportion as the exertion of the ciliary muscle is prolonged or suspended. The pain, too, is generally referred to the eyeball, or to the parts immediately around it. In the hysterical form, however, the pains are exaggerated and perpetually shifting, sometimes localised in one situation, and sometimes in another. They come and go independently of any particular exertion of the eye. Indeed, these patients give up work altogether, and profess themselves unable to do anything. There is no tenderness on pressure over the track of any particular branch of the fifth nerve, as there probably would be in the case of an ordinary neuralgia. The symptoms are much aggravated by bright light, and by any cause which in any degree disturbs or agitates the nervous system.

Such cases used to be described under the head of hyperæsthesia oculi, and one of their very remarkable features is the variation in severity which occurs from time to time. Sometimes these patients will feel very well, and at others, for no very evident reason, will express themselves as feeling most miserable, and are evidently in great distress.

A special study of cases of this kind has been made by Dr. Foerster of Breslau, and he has been able in many instances to connect these symptoms with disorders of the uterine functions. No doubt many ordinary cases of asthenopia are much aggravated in seasons of disordered health; but the very severe cases which we sometimes meet with are so tedious, and unmanageable by ordinary means, that it is a real gain to be able to trace their origin to any structural lesions such as Dr. Foerster has described.

Not many years ago, young men, wretchedly out of health in many ways, were frequently seen in the out-patient rooms, who at times could hardly open their eyes, while at others they were tormented and made miserable by the apparition of all kinds of muscæ volitantes. Many of these patients were suffering from long-standing gleet, and very often, rightly or wrongly, were

supposed to be the victims of self-abuse. Such cases were at any time most difficult to treat, and it is fortunate that it is very rarely nowadays that any such present themselves.—*St. Bartholomew's Hospital Reports, vol.* xiii., 1877, *p.* 172.

64.—A NEW TREATMENT FOR DILATING OR RENDERING PERVIOUS THE EUSTACHIAN TUBE, AND FOR VENTILATING THE TYMPANIC CAVITY.

By Dr. J. Gruber, of Vienna.

The chief difficulty in catheterising the Eustachian tube (an important operation, which I here declare will *never* fall into disuse) is well known to arise from an impervious state of the nasal fossa of that side. The muscles of the nose and the septum nasi, which contribute chiefly to form the nasal passage, are so different in size, form, and situation, they affect the opening in the nose in so many ways, and so interfere with catheterisation, that besides delicate sensibility in the fingers, which especially is essential in a good operator, great practice and experience are required before one can properly use this operative measure. It is not easy to acquire such handiness; and the aversion of patients to this instrumental treatment increases with the thought that deep and invisible parts in the neighbourhood of the brain will be operated on, the result of which is that very few medical men yet understand how to use the catheter. Further, we have to consider that there are patients whose courage fails them at the thought of permitting the instrument to be used, and there are cases where the passage of the catheter through the nasal fossa is hindered by congenital or acquired deformity, or is rendered impracticable and unadvisable on other grounds. Hence the patient and physician endeavour to carry out the indicated treatment without having recourse to these means. One of the chief ends we try to attain by the introduction of the catheter is the passage of air through the Eustachian tube into the middle ear, a method of treatment lately made known under the name of air-douche, and notoriously more frequently employed than any other in the treatment of ear diseases.

Even for using the air-douche it was desirable to learn the use of the Eustachian catheter, though the same, or almost the same, end might be attained without the actual passage of the tube.

The method of accomplishing this end employed of late years is that first described by A. Politzer, which consists, as is well known, of the following steps. The patient takes a little water in his mouth, the surgeon passes the nozzle of the specially-made instrument about one centimetre within the nasal open-

ing, which is firmly closed upon the instrument with the thumb and forefinger of the left hand; then, while the water is being swallowed (at a signal from the surgeon), the elastic ball attached to the nozzle in the nasal opening is suddenly emptied by pressure with the right hand of the surgeon.

The generally received opinion is that at the exact moment of swallowing, when, through the contraction of the middle of the pharynx, the upper part is separated from the lower, at that moment the Eustachian tube is opened, and, the air being compressed in the nasal part of the pharynx, and not being able to escape by any other passage, is forced through the tube into the cavity of the middle ear on each side.

Notwithstanding the important contribution to therapeutics which this mode of treatment was, it had many drawbacks, to which we will return when we have depicted the new method. Later, Lucae, basing his propositions on observations on the function of the soft palate and of the Eustachian tube, made by him while carefully watching the parts when in action, in a patient suffering from great destruction of the nose, recommended that, in order to convey air into the tympanic cavity, the nozzle of the Politzer bag being passed as usual into the nasal opening, the patient should be directed to sound the vowel "a" while the air-ball was suddenly emptied. The soft palate is stretched while the vowel "a" is sounded and the upper pharynx is closed, and air can be made to pass through the tube in the tympanic cavity, as in the Politzer procedure.

I intend to refer to the method of so renowned an author in another place, and will here only briefly mention that I can myself testify to the fact that it succeeds, in many cases, in accomplishing the desired end, and that, as the author himself has stated, the manœuvre may be made useful in the treatment of little children. It is to be remembered, however, that in the case of these latter, simply emptying the Politzer ball will introduce air into the middle ear.

In treating adults, especially when, as happens in very many cases, we have to overcome a marked obstruction in the tube, this method of Lucae will probably not succeed. We have generally to deal with cases of morbid change in the tube, and in such the slightest stretching of the soft palate produced by the phonation of the vowel "a" will be easily overcome by the pressure of the air, which will escape downwards without having reached the tympanic cavity.

For the air to press into the tube with the necessary elasticity, it is quite indispensable that the closure of the upper pharyngeal cavity should be capable of making firm resistance, which may be easily produced by swallowing or analogous movement; but as I found many drawbacks in the use of the swallowing move-

ment, as employed by Politzer, I devised a substitute, which I believe to be better for our purpose.

I will now proceed to a description of the process itself.

As has been already said, in order to obtain an effective separation between the upper and lower parts of the pharynx, the muscles of the soft palate must be brought into play at the same moment that the Eustachian tube is opened. All this is obtained by the simple pressure of the root of the tongue upon the hinder part of the palate, if a strong expiration is made at the same moment. If one presses the posterior part of the tongue against the palate, the cavity of the mouth is shut off from the throat, and the soft palate is pressed upwards and backwards. The air, which passes in expiration into the throat, has no escape either through the mouth or through the nose, of which fact one can easily convince oneself by holding the hand, or a small flame, in front of the nose. The latter is not moved, and the hand is not conscious of the least breeze during the expiration, as would be the case did the air escape from the nose. The stronger the expiration at this moment, the more tense will be the soft palate by the pressure of the escaping air, and the more effective the closure of the upper pharynx. This moment, as regards the arrangement of the pharyngeal parts, is the most favourable for giving the maximum degree of pressure to the pent-up air, by emptying the Politzer ball into the nose by the nozzle introduced as usual.

Had we always quite docile patients before us, we should certainly make use of this method; as this is not the case, we must be content with such movements as come nearest to that above described. The result of the investigations which I have made in my own mouth, and in those of patients, is that we obtain the result when the consonants "h," "k," "k," are sounded together in the most sudden manner. In such a mode of operation, much depends upon the patient's powers of comprehension, and it is often easier for the surgeon to direct the patient to repeat some complete syllable, as it will demand less explanation to make them use a vowel between the consonants, as "hack," "heck," "hick," "hock," "huck." Let anyone utter the indicated syllables in succession, as they are written down, and he will find that the tongue is pushed further backwards, and more firmly upwards, the further we proceed in the succession of syllables, so that with the syllable "hack," the tongue is placed most forward, and with the syllable "huck" is pushed back to the furthest degree, and against the parts above; and in this way the upper pharynx is narrowed, and effectually closed. The backward and upward pressure is stronger, and the closure more effectual when the combination of consonants "hck" is uttered without the vowel. We have

also in the scale of words a means of measuring, a kind of gauge of, the closure of the upper pharynx, which is most useful, as will be seen. As the learned reader has already observed, the syllables are always written "ck," which I will explain by saying that the strong final sound falls upon "k." In proportion as the patient exerts himself to strengthen the sound of this final "k," so will it be possible to perfectly close the upper pharynx. Let us now try to utter one of these syllables, and we shall observe peculiar changes or effects in the ear; each time we find a motion in the tympanum, and not unfrequently a noise similar to that experienced in the Valsalva experiment; and the experimenter must thus cause the air to pass through the tube into the tympanic cavity.

The treatment which I now, supported by the facts given above, recommend for cases alluded to in the preliminary remarks is as follows:—The operator stands or sits, as is most convenient for him, in front of the patient, and the end of the nozzle of the syringe (the ball of which is held in one of the operator's hands) is passed to the depth of one-third of an inch into the nasal opening. The operator then, with the thumb and first finger of the other hand, closes the opening around the syringe nozzle most carefully, and, while the patient utters one of the prescribed syllables ("hack," "heck," "hick," "hock," "huck," "hck"), the ball is compressed, and the air flows with a clearly perceptible noise through the tubes into the tympanic cavity.

(For this, my treatment, I use an instrument which differs from Leiter's in that the nozzle is solid and is screwed into the ball; in Leiter's syringe there is a piece of elastic tube between the ball and the nozzle, which I find very disadvantageous, as the ball is not so easily handled, and the nozzle cannot be kept so firmly introduced.)

The sound which is produced when the air enters through the tubes can be heard distinctly enough with the aid of the otoscope. If the tympanum is perforated the characteristic perforation sound can be heard; the patient has a clear sensation of the passage of air into the middle ear; the tympanum shows, on inspection, the well-known phenomena; and, in short, there are all the signs of entrance of air having taken place, if the tube of that side be not impermeable, in which case any injection of air would be impracticable. If I allow the patient to incline his head strongly towards one shoulder during this treatment, it is always successful, especially if I pass the nozzle into the nostril which corresponds to the ear into which I wish to inject the air. In those cases in which the air came into the ear of the other side as well, it almost always happened that the patient could feel the passage of air more

strongly in the upturned ear. If the patient, treated in this way, once perceived certainly that the air entered the ear, he had the same perception if I repeated it several times one after the other, or on different days.

Whether this posture causes the muscles of one side to act more energetically, or whether the compressed air takes the upward direction more easily, or whether air expired in this position has a stronger draught along this side of the palate, and so leads along the compressed air in its course to the nose, I cannot explain.

I should like to have made these last observations with all reserve, while I indicate of what inestimable value would be the discovery of any manipulation which would enable us to send air through one tube merely.

To sum up the advantages of my method over that of Politzer:—

1. It is more simple. The swallowing movement being abolished, the sipping of water is no longer necessary. One needs to learn the objection patients feel to this water drinking in order to estimate the boon it would be no longer to need it. Although I have used this method of treatment much less frequently than others, still there always stood on my table a great number of glasses ready, and it often cost a patient a great effort of will to take a sip out of my glasses. How can you blame him when you remember that one's consulting-room is at all times receiving patients suffering from contagious diseases?

It is easily understood that when the treatment is needed frequently, the swallowing of so much water is not only troublesome but hurtful. If it is replied that the patient could make the swallowing movement without water, I would reply that then the introduction of air through the tube often fails; and, on the other hand, that the frequent empty swallowings are more painful than when water is used. I will not mention cases in which diseases of the neck render deglutition difficult further than to say that, while attempting to swallow, air passes into the stomach, and causes severe pain, until several eructations have relieved the organ of the injected air.

2. Is one able to keep the pharyngeal parts in the position assumed in uttering the prescribed syllable, especially the "k," then the air can for a longer time be made to enter the tubes than could be the case with the brief act of swallowing, which cannot possibly be prolonged, and I hope it will some day be possible to direct medicaments into the tube in this manner.

3. By using the above list of syllables carefully the air may be made to enter the tubes with varying degrees of force, which advantage will appear more clear when it is mentioned that the

suddenness of the action in the Politzer method has often led to tympanic rupture.

4. My method is especially adapted for self-treatment, because the patient learns by his own sensations to increase the force with which the current is sent into the ears.—*Lancet, Jan.* 12, 1878, *p.* 45.

65.—FOREIGN BODIES IN THE NOSE AND EARS.

By Francis Mason, Esq., Surgeon and Lecturer on Anatomy at St. Thomas's Hospital, &c.

Foreign bodies, such as cherry-stones, locust-beans, brass rings, slate pencils, screws, buttons, pieces of wood, peas, &c., are not unfrequently met with in the aural and nasal cavities of children, and even in adults. Such substances have been known to remain in one or other of these cavities for well-nigh a lifetime, causing little or no inconvenience. Thus a case is related of a lady from whose nostril a foreign body was dislodged during the act of sneezing; it was found to be a button which had belonged to her little brother when they were both infants. Another case is recorded in which a piece of slate pencil was removed from a woman's ear, which had been put there when she was at school "forty years before." And a third instance, a patient of Mr. Winterbotham's, of Cheltenham, in which a cherry-stone had been in the ear for sixty years. Mr. Hargood, of Eastbourne, also recently reported the case of a gentleman, aged forty-one, from whose ear a piece of cedar wood was removed by syringing. "The patient remembered distinctly the fact of its introduction when he was a boy at school, at least thirty years before. No attempt had been made to extract it, and its presence had not troubled him until now."

It occasionally happens, however, that a good deal of inflammatory action is set up by the foreign body. As a case in point, I may mention that of a girl who was under my care at the hospital, and who was admitted on account of having a small stone in her ear. She subsequently had paralysis of the facial nerve. Mr. Jonathan Hutchinson alludes to the case of a child who not only had facial palsy, but died of meningitis caused by the presence of a locust-bean in the ear.

There are various instruments employed for removing foreign bodies from the ear, each good in its way. A loop of wire, a scoop, or a needle with the point just slightly turned up, forceps, or an instrument such as this I now show you—which is the best—may be used.

It is not always easy to detect the exact character of the foreign body, and as bearing on this point I may incidentally

refer to the case of the man who is here this evening in the adjoining room. About fifteen months ago he was accidentally shot behind the left ear. The patient was seen by my friend, Mr. Henry Jacobs, of Kensington, who has given me the opportunity of examining him with reference to the lodgment or not of the bullet, for the latter could not be discovered after the accident. There is a constant discharge from the ear, and, as you will observe, well marked facial palsy. My colleague, Dr. Stone, has kindly constructed a clever electrical apparatus by which we are enabled to detect the unquestionable presence of the bullet. Dr. Stone has done us the honour to be present, and I think you will agree that the Society is much indebted to him for the trouble he has taken in bringing the apparatus here, and of thus giving the Fellows the opportunity of witnessing so interesting an experiment.

[The apparatus constructed by Dr. Stone consisted of two fine pieces of silver wire, covered with silk, which were wound together in a single strand to imitate a probe, about three inches in length. The whole was insulated and stiffened with shellac, the ends being left loose for connexion with a battery and galvanometer. The joined ends at the opposite extremity were cut, so as to leave two little discs of clean silver. On placing these discs in contact with the foreign body in the ear, the galvanometer was visibly deflected, indicating the presence of a metallic substance, which was naturally assumed to be the bullet. The exposed surfaces of the discs were intentionally kept as small, and the insulation as perfect, as possible, to prevent the transmission of a perceptible current by the fluids of the sinus.

Living larvæ have been found in the meatus of the ear. Dr. Routh publishes such a case. The patient was a gentleman who three years before was tormented wito a fly near his ear. Convulsions followed the presence of the larvæ, but the patient recovered, although he remained deaf. Dr. Blake, of Boston, has seen four such cases.

Dr. Kealy, of Gosport, reports a case to show the curious course taken by a pin that had been introduced into the external meatus. It passed through the middle ear, probably along the Eustachian tube, and was extracted by the patient from her throat by hooking it with her finger.

In dealing with foreign bodies situated in the external auditory meatus, syringing the passage will often suffice to effect removal, but in many cases forceps and other instruments must be used, yet they should be employed with the greatest caution. As a rule, if left alone, the foreign body becomes loose, and falls out on the pillow as the patient lies in bed. In extracting foreign bodies from the ear, M. Debout has recommended that

the mouth of the patient should at the same time be kept open. It is sufficient, he remarks, to introduce the end of one's finger (and the Fellows may try it on their own person) into the external auditory canal, and to make the lower jaw move, in order to become convinced of the enlargement that the canal undergoes each time the condyle of the jaw is made to move. Dr. Voltilini offers some very sensible and practical remarks on this subject, which I venture to quote. He says: "The first thing we have to do is to assure ourselves that a foreign body is really in the ear, for it by no means rarely happens that persons apply to the surgeon under the belief that an insect or other substance is present, but which a more exact inspection fails to discover." He adds we should never employ force, and in saying this he did not wish to convey the idea that foreign bodies should always be left in the ear, but that matters should not be made worse by violent manipulations. Still more recently, Mr. Dalby has laid down a very practical law that no attempt should be made to remove a foreign body from the ear unless the auditory canal be thoroughly illuminated. Where this rule is broken the tympanic membrane will most probably be ruptured, and thus the life of the patient placed in imminent peril.—*Lancet, March* 9, 1878, *p.* 338.

SYPHILITIC AFFECTIONS.

66.—ON THE THERAPEUTIC USE OF IODOFORM.

By Berkeley Hill, Esq., M.B., F.R.C.S., Surgeon to University College Hospital, and to the Lock Hospital.

Locally, iodoform, as a dry powder, brushed lightly over the surface with a moistened camel-hair pencil, has been for three years my almost invariable treatment of venereal sores, especially the local chancre. During the last few months I have often substituted for the dry powder an ethereal solution (one part of iodoform in six or eight of ether). The sore is touched or dabbed with a pencil dipped in the ethereal solution, according to its size and depth, lightly or copiously. The ether quickly evaporates, leaving a thin pellicle of iodoform, that as effectually stays the spread and produces healing of chancres as does the more copiously applied dry powder. Thus the surface is covered more exactly, and the disagreeable smell of the iodoform is too faint to attract attention. The sore is well washed with water and dried before the iodoform is applied, and the surface is lastly protected by a bit of dry lint. When the secretion is abundant, the dressing must be renewed twice daily, but in three or four days the amount of discharge becomes so scant that one dressing *per diem* suffices.

In this way venereal sores heal quickly. Pain subsides at

once; the sore is well in a week or ten days, and the chances of consecutive inoculation or bubo are greatly lessened. In a very few cases, the application of iodoform gives momentary smarting, which is very bearable; even the ethereal solution does not hurt, and usually the patient declares the application to be quite painless. I avoid using iodoform on inflamed sores, or on simple granulating wounds; but indolent non-specific ulcers are rapidly improved by iodoform locally applied.

Lately, I have given iodoform internally with great benefit. It acts more rapidly than potassic or other iodides, and, judging from experience thus far, is as readily borne as are those salts. I have given it in one-and-a-half grain doses as a pill with extract of gentian. Three pills are given each day, increasing gradually till eight or ten pills are taken in twenty-four hours.

I have used it with excellent effect in cases of obstinate syphilitic ulceration of the tongue, where the dorsum is covered with rugged thickened epithelium, which is constantly splitting into deep fissures, and thus causing continual severe pain to the patient. This affection is often quite insensible to mercury, alkaline iodides, or arsenic—the remedies usually beneficial. In three of these obstinate cases, where I had been treating the patients at intervals for years with the remedies just mentioned with little lasting benefit, iodoform-pills have acted like a charm. Pain, immediately lessened, in two or three days ceased wholly; and the fissures healed rapidly, while the tongue soon shrank to its natural size. How long the relief will endure, time alone will show; but any interval of only apparent cure of this very painful affection is a great blessing to the sufferer, and time is given for the exhibition of mercury if required. In December last, I had under my care in University College Hospital a patient with ulcerated and protruding gumma of the left testis, non-ulcerating gumma of the right testis, and ulcerating gummata of the skin over the upper end of the right tibia, with other syphilitic affections. Iodoform was administered in pills, and water-dressing applied to the ulcers. Rapid healing and subsidence of the swellings took place, notwithstanding that, when the dose of eight pills *per diem* had been reached and administered for three days, an outbreak of pyrexia, coryza, and iodic acne rendered it necessary to drop the drug completely for a short time. In three weeks, the patient left the hospital almost healed, and continued his treatment as an out-patient. Again, a lady, who has during the last two years consulted me occasionally for intensely agonising pain in the head caused by syphilitic pericranial and cranial disease, for which a customary dose was thirty grains of sodium iodide three times daily, was at once relieved of pain by the iodoform pill taken three times daily, though, on the third day, nausea became too urgent to allow the iodoform

to be continued in that quantity; it was at first diminished till pain ceased, and then discontinued altogether. This small experience has satisfied me that in iodoform we have a very useful addition to our store of weapons for fighting syphilis. Further observation will enable us to apply it more exactly and when most suitable.—*British Medical Journal, Jan.* 26, 1878, *p.* 127.

67.—ON THE USE OF THE CALOMEL VAPOUR BATH.

By HENRY LEE, Esq., F.R.C.S., Surgeon to St. George's Hospital.

In the American Practitioner for September, 1877, Dr. Yandell has given perhaps the fairest and most impartial account of the different modes of using mercurial fumigation that has yet been published. It is twenty-three years, Dr. Yandell remarks, since he commenced the use of the mercurial vapour bath, and he has used it ever since. Other forms of mercurial treatment are also employed, but, where circumstances permit of it, he prefers that to any other.

Dr. Yandell commenced his experiments with what he conceived to be Mr. Langston Parker's apparatus, with the grey oxide of mercury; but found that the degree of heat necessary to vaporise the powder a very serious objection. The bisulphuret was next tried, without benefit. The irritating fumes of the sulphur and the heat acted injuriously. It was then found that the so-called cinnabar that he used contained ninety per cent. of lead to ten per cent. of mercury. Unadulterated cinnabar was now used, mixed with the grey oxide, and the results obtained were more satisfactory than with either alone. Still the extreme heat necessary to vaporise the latter, and the suffocating fumes of the former, told heavily against their use. At length calomel was tried, and "the mere mechanical troubles with the fumigations were now virtually at an end."

Dr. Yandell found, however, that his patients did not improve so rapidly as mine did in London, and asks, with much point, what the explanation can be? We both used the same apparatus, and the same quantity of calomel, and why should the same treatment cure quickly in London, and not cure quickly in Louisville? The solution of this question is not difficult, and for the benefit of others I wish to answer it publicly rather than in a private communication.

The great majority of those at first treated were hospital cases, and, as Dr. Yandell says, the London Lancet of that day abounded in reports of such cases. He gives me the credit, which I also claim, of having reported the cases faithfully. The patients, often several in succession, were placed in a box in which the ten or fifteen grains of calomel was volatilised.

The room in which the box was contained was small, and, looking back upon the rapid and almost uniform results obtained, I have no doubt whatever that the patients got the benefit of some of the calomel that was left in the box, and perhaps in the room, in addition to the ten or fifteen grains that was devoted to their individual use. In private practice I generally directed patients to use the same cloak night after night, and to sleep in it, and thus the calomel vaporised one night was again to some extent utilised the next.

Dr. Yandell found that in order to produce the desired effect he had often to use one scruple, half a drachm, or a drachm of calomel for each bath. Where patients like to have a clean cloak for their baths, and wash the calomel off by means of baths, I have, as he suggests, found the same thing, so that substantially his experience and my own coincide. It may be well here to mention that I now use calomel that has been previously resublimed two or three times. Ordinary calomel is less affected by heat or moisture than any other preparation of mercury, but still it does contain a certain amount of hydrochloric acid, the presence of which may be indicated by a piece of moist litmus-paper held in the fumes as they arise. This free hydrochloric acid is driven off in a great measure by sublimation, and the pure calomel thus prepared is less irritating than the ordinary calomel of commerce. It should also be observed that the water I originally used was principally for the purpose of preventing irritation from any fumes that might be generated during the action of the baths, and I find that an ounce on each occasion is quite sufficient. If more water be employed, more heat is necessarily required in order to boil it. The vapour of the water is in part deposited on the patient's skin; this must in some way be removed before he is comfortable, and some of the calomel is necessarily removed with it. Dr. Yandell uses a pint of water in the apparatus which he has depicted, and the patient has thus a combined vapour and calomel bath. This no doubt may be very useful where such a combination is intended, but the effect is often very different from that produced by the calomel bath alone; a much greater amount of perspiration is induced, and this the patients, when the bath is repeated night after night, cannot bear. The perspiration also tends to remove the calomel from the skin.

Dr. L. P. Yandell is of opinion that brisk friction after the sweat, made with the coarsest towel, and until the skin is all of a glow, actually promotes the action of mercury, and conduces to its more rapid absorption. This no doubt may be the case, but it involves a different principle. The calomel is rubbed into the skin in a similar way as the mercurial ointment was in olden times.—*Lancet, Feb.* 9, 1878, *p.* 193.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

68.—ON TWO CONTRASTED FORMS OF WEAK LABOUR.

By Dr. J. MATTHEWS DUNCAN, Hon. Fellow of the Obstetrical Society of Edinburgh; Obstetric Physician to St. Bartholomew's Hospital.

The commoner kind of nearly powerless labour arises from inertia of the uterus. It is most characteristically observed in multiparæ who have had numerous children and are elderly. The uterus does not become stimulated to sufficient activity. The child's head may have reached the perineum, and makes no further progress. There is no urgency, perhaps, in the case; but the mere lapse of time renders delivery desirable. The inefficient pains, with the accompanying inefficient bearing-down efforts, are supplemented by traction with the short forceps, or by expression by the accoucheur's hands on the fundus uteri; and the child is brought into the world by a very small expenditure of force. Assumption of the erect position or voluntary bearing-down may be enough, in some such cases, to complete a delivery which the uterine pains have failed to effect. After the child is born, retention of the placenta and hemorrhage are very liable to occur.

The rarer form of weak labour is, in most respects, a contrast to the former. So far as my observation goes, it occurs chiefly in primiparæ, or young women who have a special nervous mobility. It is generally, but erroneously, included with the former kind under uterine inertia. The uterus is unduly, but morbidly active. Its tonic or permanent contraction, or its retraction, goes on with premature and injurious rapidity. The intermittent contractions or pains continue, and are painful, and force complaints from the mother, but they are inefficient, and may be justly called spasmodic. The uterine body, which covered the whole fœtus as far down as the brim of the pelvis—and had, in order to form such covering, a deep cup-like shape, the rim being attached to the expanded cervix—is now a mere cap or prolate dome-like covering of the lower fœtal parts. Its fundus is higher in the abdomen than it otherwise would be, and it extends downwards over the fœtus only to the extent of two or three finger-breadths below the navel, or even less. The

pains are severe enough, but inefficient from want of scope of contraction. In this case the practitioner finding the head, we may suppose, near the perineum, expects difficulty and experiences none. Delivery is effected by the same means as in cases of the former category and with equal ease. After the child is born the placenta is easily extracted or expelled and hemorrhage is very unlikely to occur—just as it is unlikely to occur from the much and long retracted uterus of an ordinary tedious labour from mechanical obstruction.

I may here mention the joy with which I first, and not very long ago, recognised the latter class of cases. The patient was excessively nervous. Though healthy and of healthy family, she lived in a morbid fear of disasters, which was justified by no circumstance, and which nothing could even temporarily dispel. She had no deformity of pelvis, no rigidity of soft parts, no deficiency of uterine pains apparently strong; yet, when the head reached the perineum, while the pains continued severe, progress was arrested. In her first two labours I could not divine the cause, and this was the basis of my statement to the husband and friends, a statement as unsatisfactory to myself as to them. In her third labour the same course of events occurred, but then I carefully palpated the uterus; and, during a pain, easily diagnosed the lower margin of the uterine body little more than an inch below the umbilicus. The case was clear to me, though not so to the friends, who, however, were naturally quite satisfied with my declared recognition of its real nature. Delivery by forceps was effected without any effort worthy of the name of pulling: the child was little more than lifted out of the passages.

Weak labour from inertia of the uterus before the birth of the child is to be distinguished from nearly powerless labour arising from premature uterine retraction by the following circumstances:—

In the former, the pains are generally seldom, short, and cause little suffering. In the latter, they may be frequent and of ordinary duration, and painful. In the former, bearing down is generally slight or absent. In the latter, it is unaffected or powerful. In the former, the uterus proper is flabby, and its feeling under the hand during a pain is never that of great tension. In the latter, it is quite otherwise. In the former, the uterus is deficient in irritability under kneading or friction. In the latter, it is otherwise. In the former, the lower margin of the uterine body cannot be felt at all, or is indistinctly perceived immediately above the symphysis. It cannot be felt and recognised with ease. In the latter, it is rapidly elevated to near the umbilicus as labour goes on, and is comparatively easily felt; its hard and somewhat rounded edge

marking a limit between it and the cervix; the former hard and firm, and allowing nothing to be felt through it while pain lasts; the latter thin and tight during a pain, and even then allowing the fœtal parts to be felt through it.

Retraction of the uterus before the birth of the child cannot with propriety be described as premature when it is observed as it occurs most frequently—that is, in the course of labours in which the progress of the child is long obstructed. Then it arises from the well understood failure of natural and powerful uterine contractions to propel the child, while the natural continuous contraction produces its ordinary result, namely, retraction of the uterine body. This retraction is accompanied by elevation of the uterine fundus, and by dangerous elongation and thinning of the uterine cervix and adjacent vagina. In the cases which I have been describing there is no special difficulty in propelling the child, no obstruction, yet the continuous contraction goes on with rapidity. The retraction is truly premature, resulting in the development of a powerless labour before the process, otherwise natural and easy, is finished; resulting also in the tensile elongation of the cervix uteri and vagina, with the well-known dangers of that condition—dangers, I may add, which Braxton Hicks has well illustrated in his paper on laceration of the vagina, published in the Lancet in 1869.

The importance, with a view to management, of timely recognition of the nature of a case of simply weak and ineffectual labour does not need to be insisted upon. The pathological conditions are so widely different in weak labour from inertia from what they are in weak labour from premature uterine retraction, that the same treatment cannot be expected to be applicable to both. Besides, the dangers to mother and child both before and after delivery are quite different in the two sets of cases.

In inertia, the uterus is to be stimulated by oxytocics, and the most powerful of these, ergot, may be used with advantage if the completion of delivery will not be long delayed. The child is in no danger from the continuance of the labour, and the slowly increased retraction of the uterus produced by ergot interferes little with the expulsive forces; and, after the birth of the child, will tend to make the third stage healthy and without dangerous hemorrhage. If the ergot increases the pains or temporary uterine contractions, such increase can only be beneficial. Rapid or early delivery, however, is to be deprecated. The easiness with which the child is propelled is a chief source of danger, and it may be advantageous to obstruct delivery, for a few pains, by perineal pressure, in order to stimulate the uterus to more and yet more powerful action.

In premature uterine retraction the uterus is not to be stimulated, but soothed if possible. Opiates and chloroform may be of service with this view. Uterine oxytocics of all kinds are to be avoided; they will increase the evil and the danger, not inconsiderable for both mother and child; the danger to the former being connected with tensile elongation of the uterine cervix and vagina; to the latter from compression and condensation, if not separation, of the placenta. Early delivery is desirable as soon as the inefficiency of the natural powers is demonstrated, for nothing but aggravation of the case results from delay. The urgency of the case is matter of actual measurement in one important respect. The more the uterus is retracted, the greater is the thinning of the cervix, and the greater the danger to the mother and child. The degree of retraction can be measured by ascertaining the distance of the lower border of the body of the uterus from the symphysis pubis on the one hand, and from the elevated fundus on the other.

With a view to simplicity, I have considered cases only which are uncomplicated. In both sets of cases it may be necessary to complete delivery artificially. In the easiest, expression by the method of Kristeller and others may be effectual. In any case, the forceps will not fail.—*Obstetrical Jour., Feb.* 1878, *p.* 705.

69.—ON THE TREATMENT OF AFTER-PAINS.

By Dr. Bernard Kelly, Medical Officer of the Fifth District of St. Olave's Union, Southwark, London.

I have always, since I have known anything of practical midwifery, deemed the hasty interference with after-pains as highly injudicious. What labour-pains are directly to the fœtus and placenta, the after-pains are to the mother. Let either be incautiously tampered with, aud danger will almost inevitably ensue. Now, let us ask ourselves the question: What purposes do the after-pains fulfil? That they expel clots remaining in the cavity of the womb, and reduce the organ to something like its ante-gravid state, and prevent, if not check, uterine hemorrhage, there can scarcely be a rational doubt. They also seem to be a continuation of the natural labour-pains, and, as such, tend towards gradually expending the nervous energy centered in the uterus, but more especially in the hypogastric plexus of the great sympathetic; and ought, therefore, not to be lightly interfered with, or suddenly suppressed.

The wombs of primiparæ, as a rule, being endowed with greater tone and muscular contractility than those of multiparæ, resume more quickly their natural dimensions and position in

the pelvis; indeed, they may be said to do so almost directly after the labour is ended. Hence the general absence of after-pains in the case of the former, and their very great frequency in that of the latter class.

If after-pains are due to the presence of clots in the cavity of the uterus (which, however, I very decidedly doubt), then Nature provides a remedy in uterine action which removes them far more effectually, and with much less risk and danger to the mother than can the hand of the accoucheur; which, to say the least of it, is a very cumbrous and repulsive expedient.

And now as to the treatment. If after-pains are a continuation and supplement, as I believe them to be, of the normal parturient process, then the less we meddle with them the better. This expectant method I have found in the great majority of cases to be both safe and strictly correct. It is only when they continue unusually long, and are very severe, that remedial measures are called for. Here I have found small doses of opium frequently repeated, when no uræmic complication co-existed, combined with an alkali or acid, as either was indicated, to answer an excellent purpose. This plan I consider much safer than giving the drug in a single large dose, which has a tendency to suddenly paralyse nervous action, and to subsequently lead to passive enlargement and congestion of the uterus. This objection derives additional force from the occasional presence (often unsuspected by the young accoucheur) of albumen in the urine, and the consequent danger of uræmic convulsions; when, if a large dose of the narcotic be blindly administered, fatal effects will almost inevitably follow. Here there is no remedy to compare in safety and value with the old-fashioned tincture of iron, which may be given, properly diluted, in large doses and *ad libitum*. When pains, unattended by uterine action, follow labour (which, in my experience, is very rare, unless as the prodromata of puerperal peritonitis), why then ergot may be advantageously prescribed. But, to exhibit it in cases where the viscus is already, so to speak, madly in action, would be simply applying spurs to the willing horse. In dealing with this affection, we should ever be guided by coolness and common sense; and bear in mind the apothegm of a great authority on the subject, "Meddlesome midwifery is bad."—*Medical Press and Circular, Feb.* 20, 1878, *p.* 146.

70.—TUPELO TENTS FOR DILATING THE OS AND CERVIX UTERI.

These tents have been introduced into England by Messrs. SALT and SON, Birmingham, who have received most satisfactory reports from members of the profession, who have tested them.

The material of which these tents is composed is the root of the Tupelo tree, which grows in swamps and wet places in Georgia and Florida. It consists of finely grained, but soft woody tissue, the fibres not being straight but interwoven, and collected in bundles. The tents are cut of the required lengths and from four to ten times thicker than they are intended to be for introduction, and are reduced to the proper size by hydraulic pressure. They are sometimes made hollow with internal metallic tubes to admit of the escape of fluid during the menstrual period, as advocated by Dr. Sussdorff in the New York Medical Record.

The following are the advantages possessed by these tents over those made either of sea-tangle or sponge:—

1. Being smoother and firmer they are much more easily introduced. 2. Being very light and absorbent, they quickly receive sufficient moisture to expand them and keep them *in situ*. 3. They are probably themselves of antiseptic nature, or at least will not decompose the fluids with which they come in contact. 4. They have none of the offensiveness always accompanying sponge and sea-tangle tents, and are not likely to induce septic poisoning or local irritation. 5. The rapidity with which they expand when in contact with the secretions of the uterus is one of their chief advantages, the time required for complete expansion varying from one hour in the smallest to six hours in the largest. 6. The firm and even pressure they exert is calculated to effect a beneficial alterative influence on the tissues upon which they operate. 7. The difficulties encountered in removing sponge and sea-tangle tents are obviated in this material; they will not break nor fracture when dry, nor when moist will they peel off and leave small particles adherent to the mucous membrane, as sometimes happens with the other kinds. 8. Their dilatable properties may be roundly stated as from the size of a goose quill to that of the diameter of a sixpence.—*Communicated.*

71.—ON THE USE AND ABUSE OF PESSARIES.

By Dr. George Granville Bantock, F.R.C.S. Ed., Surgeon to the Samaritan Free Hospital.

So various are the views held as to the value of mechanical appliances in the treatment of uterine deviations, that while, on the one hand, many eminent gynæcologists regard pessaries with great favour, as very efficient aids in the treatment of deviations, and the morbid conditions so frequently associated with, if not actually dependent on, them, others equally eminent are as much opposed to them, attributing to their use untold mischief.

At the Manchester meeting of the British Medical Association Dr. Braxton Hicks read a paper "On Hemorrhage from the Retroflected Uterus, and its Treatment," in which he urged that the weighty and engorged fundus uteri was most relieved by mechanical support, at the same time combating the objections to this mode of treatment, and he quoted cases in illustration. At the same meeting, Dr. Thos. Chambers read a paper "On the Treatment of Uterine Flexions by the Intra-uterine Stem, with Cases." Dr. Henry Bennet "felt bound to state that his whole experience was antagonistic to the doctrines and treatment recorded in the paper read." Dr. Matthews Duncan agreed with Dr. Bennet, adding that "there was a fashion in these matters. Years ago every woman suffering from uterine disease was said to have a dislocated uterus; at a later period no one had any affection of that sort; and now, once more, every woman was getting her uterus dislocated again, and he defied all the doctors in Christendom to put it right." Such is the report furnished by the Journal, and I presume it has met with Dr. Duncan's approval. Now to quote this exaggerated misrepresentation is to refute it.

During a discussion at the Gynæcological Society of New York, Dr. Atlee, of Philadelphia, observed that he had had no experience in the *introduction of pessaries*, but that he had had a large experience in their withdrawal. He had been able to remove the symptoms in most of his patients without the use of pessaries, and when that could be done he was satisfied without their use. With the *uterus and pelvic organs in a healthy condition* a change in the position of the uterus was of no significance whatever, and there was no need of an instrument to keep it in a certain position. (The italics are mine.) Now, when Dr. Atlee tells us that he has had no experience in the introduction of pessaries, he at once puts himself out of court, and when he states that he has been able to remove the symptoms in most of his patients without the use of pessaries the obvious reply is that if he had used the pessary in the rebellious cases still greater success would have attended his treatment. Again, when he says that with the uterus and pelvic organs in a healthy condition (and I would add *and the patient free from symptoms*) a change in the position of the organ is of no significance whatever, he states a self-evident proposition. I have taken part in frequent discussions on this subject, and I am unable to recall a single instance of any one contesting this point. For what is the object of all treatment? It is to *relieve symptoms*, and the relief of symptoms is the measure of the efficacy of all treatment. A man with a dislocated shoulder, which does not interfere with the full use of his arm, would not trouble himself about reduction any more than a woman with a

dislocated uterus, which produces no symptoms, would apply for relief. Is it nothing to be able to say that a woman who walks into your consulting-room, complaining of pain in the sacral region, and an undefined feeling of "bearing down" in the pelvis, which interferes with her walking, is aggravated by a fæcal evacuation, and prohibits sexual relations, in a few minutes after the application of a pessary, walks with comfort, tells you she is now free from pain, and goes home to find that she can discharge all her duties with satisfaction? Can the same be said of any other method of treatment—the frequent rectifications of the uterus by means of the sound, or by the fingers, the repeated scarifications and leechings, and the two or three years' treatment of Dr. Henry Bennet?

Case 1.—About seven years ago I was asked by a friend to see his wife, who for months had been almost confined to her bed. She had had two children, and had never been well since her last confinement. On examination I found the uterus very large, measuring about three inches and a half in its cavity, and so retroverted that the os uteri pointed to the upper part of the vaginal outlet. The left ovary could be felt most distinctly prolapsed into the left side of the utero-rectal cul de sac, and it was very tender to the touch. The right could also be felt by bimanual examination scarcely lower than its normal position. I was also told that for many months the patient did little more than pass from her bedroom, usually in the evening, to the couch in the sitting-room, so much pain did the erect position, or the act of walking, cause her. There was no leucorrhœa nor excoriation, nor could I find any cause for the symptoms other than the retroversion of the uterus and the consequent prolapsus of the ovary. I at once replaced the uterus by means of the sound, and while it was thus kept in position I introduced a Hodge's pessary. The sound indicated that the pessary was not efficient. I therefore removed it, and passed a larger instrument, with satisfactory result. The ovary was no longer prolapsed. I asked her to get out of bed while we left the room. This she did with a sense of great relief, and I bade her good-bye, recommending her not to exert herself much for a few days. I remained a short time in the house in conversation with her husband, and before leaving had the satisfaction of seeing her walk into the room, when she expressed her delight at the success of the treatment. Beyond this nothing was done except the administering of an occasional dose of a saline chalybeate aperient. The patient came to my house two or three times (a distance of four miles) in order that I might be satisfied as to her condition. After several months the instrument was removed, the uterus and ovary were left in normal position, and the patient has continued quite well to this day.

Case 2.—Retroversion with menorrhagia.—In 1871 Mrs. B. came under my care suffering from severe menorrhagia and dysmenorrhœa for which she had been under medical treatment for several months. She complained of constant pain more or less severe, which so interfered with her walking that it was with great pain and difficulty she made her way to the out-patient department of the Samaritan Free Hospital. Menstruation was excessive in quantity and duration. I found the uterus very much retroverted, body enlarged, cavity measuring about $3\frac{3}{4}$ inches. The organ was readily replaced by means of the sound, but at once fell back on removing the support. There was tenderness of the body on pressure, great tenderness of the fundus on pressing the sound against it, and a little blood followed the use of the instrument. I at once adjusted a Hodge's pessary with my usual precautions, and the patient went home in great comfort with a prescription for tincture of muriate of iron and liquid extract of ergot, ten minims of each to be taken three times daily. From this time I attended the patient at her own home. She wore the instrument for about nine months, during which time she was able to attend to her household duties; the periods gradually assumed the normal character, assisted, as I believe, by the use of two sponge-tents, and I removed the instrument. This patient is still quite well.

Case 3.—Retroversion; severe menorrhagia; Hodge's pessary; subsequent pregnancy.—Mrs. D., aged thirty-three, came under my care at the Samaritan Free Hospital in the summer of 1875, the subject of severe menorrhagia, which told its tale in her anæmic appearance, and from which she had suffered since her last (sixth) child about a year and a half ago. She also complained of a constant bearing down, and stated that the loss of blood was very great, and she was scarcely a week free from a hemorrhagic discharge. I prescribed iron and ergot. A few days afterwards I was requested to visit her at her own home, and so great was the loss that I at first thought I had to do with a case of abortion. I then found the uterus very much retroverted, and prescribed ten grains of gallic acid every two hours. As soon as possible I admitted her into the hospital, and on the same day I adjusted a Hodge's pessary. This gave immediate relief to the feeling of bearing down. I kept her in bed for about a fortnight, administering iron and ergot three times a day, with the result of procuring her an interval of nearly three weeks and a moderate period. I then dismissed her. She returned on Nov. 9th, stating that the menses were regular and not excessive in quantity, the flow lasting eight days "off and on." She complained of some discomfort in the left groin. Uterus found in good position, well

supported by the pessary. The bowels were constipated, and she had frequent headaches. I prescribed quinine and iron and a mild aperient every night. On Dec. 7th I substituted for this a saline chalybeate aperient, with such effect, that by Jan. 25th, 1876, she was again free from symptoms. The last period continued for seven days, fair quantity, and, after an interval of three weeks, on Feb. 11th, she again returned, complaining of aching in the pelvic region and bearing down, and stated that she had "gone over her time." I kept her under observation till May 2nd, when I was satisfied that she was pregnant, and on the 23rd I removed the instrument. She was confined on Sept. 25th. No return of the retroversion or menorrhagia.

Case 4.—Frequent abortions, due to retroversion; menorrhagia; Hodge's pessary; subsequent pregnancy.—Mrs. H., aged twenty-four, married eighteen months, consulted me on Oct. 9th, 1872, on account of menorrhagia and frequent miscarriages, of which she had had three—the first at three months, the second at four months, and the third at two months. She complained of a feeling of weight in the sacrum and hypogastrium, increased by exertion. Menses very free, lasting eight days, much more abundant than before marriage. Patient, moreover, was anæmic in appearance; uterus retroverted; os open; uterine tissues generally flabby; slight leucorrhœa. A Hodge's pessary kept the uterus in excellent position; iron and ergot prescribed. Nov. 9th: Uterus in good position, admitting sound readily in normal direction. I recommended her to continue the treatment, and to let me know should she miss a period. Jan. 8th, 1873: Stated that she had last menstruated in the last week of November, and for the last few days had felt sick in the morning. For the last two or three days she had felt some bearing down on standing. I found the pessary lying across the vagina, but the uterus still in position. I withdrew the instrument, and while the patient was in the knee-shoulder position I reintroduced it. It will suffice to say that on Jan. 26th she had a slight hemorrhagic discharge; that on Feb. 10th I substituted (with immediate relief to pain in the sacral region on sitting or standing) a larger instrument, as the uterus was rather low in the pelvis; that from the 24th to the 27th she was again threatened with abortion; that I removed the instrument on May 22nd, and that the patient was confined on Sept. 3rd, under the care of Dr. Baxter Forman, of Stoke Newington. She made a good recovery, has had more children since, and is won, I believe, in good health.

Case 5.—Retroversion, with attendant symptoms; pessary; pregnancy.—Mrs. S., aged twenty-seven; six children, the last on Sept. 20th, 1875. She came under my care on April 3rd,

1876, stating that since her last confinement she had suffered from severe bearing down and pain in the hypogastrium, for which she had been continuously under treatment, but without relief. Bowels costive and evacuatious painful; sexual relations intolerable. I found the uterus retroverted, the fundus and body tender on pressure (with the view of elevation by the finger), and the os open, admitting the tip of the index finger; no excoriation and very little leucorrhœa. A Hodge's pessary gave immediate relief, and the patient walked home in comfort. I prescribed also a saline chalybeate aperient. On the 5th she returned to say that she was perfectly free from pain in walking, and had no bearing down. On July 1st I removed the pessary as an experiment, though the time was in my opinion too short; but the uterus remained in good position. She returned on the 8th, with the uterus again retroverted and a recurrence of the old symptoms. I reintroduced Hodge's pessary, with the same result as before. On Sept. 18th the sound entered readily in the normal direction, and there were no symptoms. Nov. 14th: Had missed her period by four days, and for several days had had morning sickness. On March 19th, 1876, I removed the pessary; and on July 20th I attended the patient in her confinement, from which she made an excellent recovery. No return of the retroversion.

Case 6.—Mrs. S., the subject of repeated miscarriages, was sent to me by a neighbouring practitioner, by whom she was supposed (from her symptoms) to be suffering from prolapsus. The case was one of retroversion, and was at once relieved by a Hodge's pessary. About a month afterwards, through violent exertion in lifting, the instrument was thrust out, and her old symptoms returned. I reintroduced the pessary. She became pregnant some months after. The instrument was worn till about the fifth month, and the patient was somewhat prematurely delivered of a double monster, which is now in the museum of the Obstetrical Society.

I could go on repeating cases *usque ad nauseam* to show the great value of this instrument in cases of retroversion. As I stated at the Manchester meeting, I regard it as a most efficient aid in the cases treated of by Dr. Braxton Hicks, and in many cases as the only treatment necessary. The preceding cases show in the clearest manner the direct relation between the displacement and the attendant symptoms. It is, moreover, with me a matter of repeated observation that in cases of subinvolution complicated with retroversion the restoration of the uterus is a *sine quâ non* of successful treatment. Common sense teaches, and experience confirms it. Of course medical treatment goes hand in hand with the mechanical, but only according to circumstances. Each case must be a law to itself, and it

is impossible to lay down such rules as will do away with the necessity for the exercise of common sense. It is true that we rarely find cases of retroversion in which no symptoms are attributable to the displacement, but that can hardly be used as an argument against the use of mechanical treatment in cases in which the symptoms are directly traceable to the displacement. On the other hand, the man who, on finding a uterus retroverted at once rushes to his stock of pessaries, and proceeds to adjust one as a matter of course, is not to be commended either for his judgment or skill.

The mode of application of the pessary is, in my opinion, a most important matter. I regard it as essential that we should in all cases, if possible, obtain positive indications that the instrument is efficient. This is best done by placing the patient on her back, and maintaining the uterus in position by means of the sound until the pessary is in its place. If the sound retains its position when let go, then we know that the uterus is properly supported, while the slightest deviation at once tells us the reverse, when the pessary must be removed, and its shape or size altered as required. I cannot agree with Dr. Braxton Hicks that it is of no consequence that the uterus should at once be accurately replaced. He thinks we should be satisfied with a partial adjustment, leaving the accurate replacement for a future opportunity. Theoretically at least this is strange teaching, and is not in keeping with his usual scientific precision. My experience is decidedly opposed to it, for in proportion to the failure in accurate replacement is the want of success in relieving the symptoms.

Difference of opinion exists as to the mode of action of Hodge's pessary. For while some hold that the support is directed through the fundus, others regard it as the result of action on the cervix. I accept the latter view in the great majority of cases, for my best results have been obtained with the long S-shaped instrument (I am here speaking of *retroversion*). It is undeniable that in some cases the posterior cul-de-sac of the vagina is so large and distensible that it is possible to afford support through the fundus; but the cases are so rare as to constitute the exception which proves the rule. I have repeatedly demonstrated in the out-patient department of the Samaritan Free Hospital that backward pressure on the cervix brings the uterus forwards, especially when aided by a suitable position, such as the knee-shoulder position, and in cases of pregnancy I have taken advantage of this fact in adjusting or readjusting a pessary. This results also from the fact that the uterus is a rigid body (in cases of *version*). This pessary, then, acts in the first place by stretching the vagina longitudinally. The result of this is that the posterior cul-de-sac is tightened, and the

cervix is drawn backwards, the body moving proportionately. The necessity of having an accurately proportioned pessary must be at once apparent, and this is well illustrated by the first case I have quoted. Moreover, a consideration of the anatomical relations of the parts leads us to the same conclusion. If anyone will take the trouble to look at, say, Savage's Plate VIII., Fig. 2, or Plate XI., Fig. 3, it will at once be seen how impossible it is, in any ordinary case, to bring pressure to bear on the fundus. Hence, as I shall show, the uselessness of vaginal pessaries in cases of *retroflexion.*

The evidence as to the value of Hodge's pessary, in all its modifications, is overwhelming, and it is now too late to take refuge in systematic pooh-poohing. And while it is capable of doing much good in skilful hands, it must not be forgotten that it is equally capable of much injury in the hands of the ignorant or careless. I am afraid it is a fact that many men pass through our medical schools, destined for general practice, without ever having seen a pessary applied. This I say as a result of experience.

There are other instruments besides Hodge's pessary of great service in uterine displacements, notably Zwanke's pessary. The flat ovoid boxwood pessaries are, I would fain hope, things of the past. Of Meadows' compound I shall have to speak further on. Zwanke's pessary is, however, an instrument of great capabilities in cases of prolapsus in the young, and affords great comfort to the aged. The facility with which it can be applied and removed by the patient is a great recommendation. It is not infrequent among the poorer class for a woman to get prolapsus as the result of getting up too soon after confinement. The easier and shorter this is, the more likely is it to happen, both being dependent on the large size of the pelvis and the dilatability of the soft tissues. I have seen the uterus in such a case, measuring four inches and a half, become reduced in three months to its normal size, and I have seen such a case cured by subsequent pregnancy with proper precautions during the puerperal period.

The abuse of the pessary may arise from several causes, which may be enumerated as follows:—(1) Improper selection of cases, (2) use of an ill-fitting instrument, (3) misuse of a properly-fitting instrument.

1. As to the improper selection of cases. In the front I place the employment of vaginal pessaries in cases of *flexion,* whether ante or retro. In the case of retroflexion, the tightening of the posterior cul-de-sac tends, by drawing back the cervix, as I have already shown, to double the uterus still more, and any effect produced on the body will simply be a rotating movement. What do we find at the bedside? The posterior trans-

verse bar passes up behind the cervix to its junction with the body (which is the site of flexion); the tightening of the cul-de-sac drags the cervix still more backwards; the pessary slips into the hollow of the flexion, especially if the instrument be not straight; and the body of the uterus topples over, aggravating the previous condition. Let us assume that the pessary has been applied while the uterus is kept in position by means of the sound. If now the sound be left unsupported, it, slowly it may be, but surely, turns round until the body of the uterus hangs over the transverse bar, behind which it can be felt by the finger. Let the sound be removed before the uterus has been allowed to fall back, and then let the sound be reintroduced, or rather, I should say, let the attempt be made, and I unhesitatingly affirm that failure will be the result. I have repeated this so often in deference to the generally received views that I have no doubt about the matter. The views I now hold are the result of the many failures I have experienced, and in proportion as I have acted on the knowledge thus obtained so have I been more successful. At a recent meeting of the Obstetrical Society I took occasion to express my surprise that the advocates of the vaginal pessary in cases of *retroflexion* were still so numerous. In this I am not without supporters.

Nor in the case of anteflexion is the result much more satisfactory. Look at Dr. Graily Hewitt's Plate 78 (second edition), and it will be seen that the highest point of the instrument is just at the point of flexion. When the bladder is empty, what is there to prevent the uterine body from bending over the instrument? But, it may be argued, this pessary relieves the dysmenorrhœa. True, it does in some cases, where there is no contraction of the internal os, by converting a flexion into a version, by the action, already described, on the cervix. But where constriction exists, it only aggravates the dysmenorrhœa, as I have proved,. Here, then, is the case for an intra-uterine stem, either with or without previous division.

2. There is no more frequent cause of failure in obtaining the full value of the pessary than in the "use of an ill-fitting instrument." It has frequently happened to me to find that the first instrument, after apparently being right when first introduced, has been inefficient. The test of this is the facility with which the sound can be introduced. It has also frequently occurred to me to have to remove pessaries introduced by others. In these cases the removal of the instrument has been necessary because of both the improper size and shape. Sometimes, however, there can be no doubt the instrument becomes too small in consequence of the stretching of the vagina. I know this has happened in my own hands.

The habit of employing hard vulcanite pessaries is apt to con-

tribute to this misuse; they are not kept in sufficient variety, and are difficult to alter. I have now for some years been in the habit of using those made of pewter. They are obtainable from Khrone and Sesemann in nine sizes. They are light, easily altered, and produce no irritation. I have known these instruments worn continuously for over a year without undergoing any change; and they have this advantage, that the presence of excoriation or ulceration is at once revealed by the blackening of the metal, which can be seen without removing the instrument.

Mrs. B., aged thirty-nine, came under my care at the Samaritan Free Hospital in February, 1873. She had been only recently married, and was complaining of bearing down, which had come on since her marriage. I found that this was due to retroversion, and I introduced a pessary, which at once gave relief. After a few weeks I dismissed her, and had forgotten her case. About twelve months afterwards she returned, wishing to know why she had missed her period. I was careful not to use the sound, and merely satisfied myself by digital examination that the pessary was in a good position. The patient had no complaint to make of pain or even discomfort, and the instrument did not interfere with her in any way. It turned out she was pregnant. In the course of the fifth month I removed the instrument, which was as clean as if she had only worn it for a day or two. In due course she was delivered of a living child, which, however, survived only a short time.—*Lancet, Jan.* 26, *and Feb.* 2, 1878, *pp.* 120, 162.

72.—CASES OF POLYPUS UTERI.

By the late Dr. FLEETWOOD CHURCHILL, M.R.I.A., Ex-President of the King and Queen's College of Physicians, and of the Obstetrical Society of Ireland.

At the request of the Hon. Secretary of the Obstetrical Society, I have put together three cases of fibroid polypus of the uterus, more from the interest I feel in the Society, and in acknowledgment of the claims it has upon its members, than because of their intrinsic value. I think, however, they are of some interest—first in being specimens of very large polypi; and, secondly, in these days of ingenious and complicated instruments, as illustrations of the value of simpler agencies.

Case 1.—Miss W., a large, healthy woman, of about forty, of active habits, had latterly been troubled with uterine discharges, red and white, and complained much of a sense of fulness and weight in the pelvis. On making an examination, I found the vagina distended to the utmost, and quite filled by one of the largest polypi I have ever met, whose neck I could barely touch

with the point of my finger. So far as I could ascertain, it grew from the cervix, and I proposed to remove it by ligature. This was some thirty years ago, before the écraseur had been applied to the removal of these growths. I had the advantage of Dr. M'Clintock's assistance, and passed a ligature secured by Gooch's canula, but with some misgiving that, owing to the small orifice and limited space in the pelvis, I had only encircled a portion of the polypus, and had not succeeded in reaching the neck. In a day or two, when I endeavoured to tighten the ligature, I found my misgivings were only too true, for the ligature slipped off. I therefore determined to remove it by the scissors, and having provided myself with a strong and long pair, with a vulsellum and the necessaries for plugging the vagina afterwards, I introduced the scissors, guided by my forefinger, up to the pedicle, the tumour having been fixed and drawn down as far as possible by the vulsellum; then changing hands, when necessary, to guard the vagina and cervix. I proceeded gradually to snip through the stalk, which was accomplished in a little time. But the difficulty was by no means over. It is true that I had separated the tumour, but it was so large and firm, and the vaginal orifice so small, that its extraction required as much force as I ever had to employ in extracting the fœtal head with the forceps. After this I plugged the vagina fully with French wadding, which I removed in twenty-four hours, and as there was no bleeding I did not plug again. Notwithstanding an attack of phlegmasia dolens about a week afterwards, the lady made a good recovery, and was alive a few years ago.

Case 2.—The second case also occurred in a maiden lady, about thirty. I may remark how very much the difficulty of these operations is increased by the smallness of the orifice through which you have to act. This lady had suffered very much from hemorrhage for a year or two, and was reduced in strength and blanched. I had to divide the hymen before I could pass in my finger, and then I found a very large polypus growing from the cervix and filling the vagina. Having secured the valuable assistance of Dr. M'Clintock, I determined to remove it by the écraseur; but, after a patient trial by Dr. M'Clintock and myself, we found it impossible to pass the wire or chain round the pedicle. Having foreseen the possibility of such a result, I had provided myself with the long scissors, and having fixed the tumour with a vulsellum, I had comparatively little difficulty in snipping across the neck and freeing the tumour, but more in extracting the polypus, after which the vagina was well plugged. On removing the plug, as there was no bleeding, it was not replaced. The lady recovered perfectly.

Now, without wishing for a moment to undervalue the écra-

seur, which I have often used with much satisfaction, I should like to impress on the members that, should it be found too difficult of introduction, we have a much simpler and easy method of operating with the scissors. But they must be long and strong, and one ought to be able to use them with either hand. It is necessary to be provided with a vulsellum or the corkscrew-like instrument invented by Dr. M'Clintock, so as to draw down and fix the tumour. The great fear, of course, in cutting across the stalk of the polypus, is hemorrhage. I think it rarely occurs to a great extent with large polypi, but at any rate we ought to be prepared, and as we can plug the vagina with cotton wool or French wadding through a speculum, without pain, and so tightly as to restrain all hemorrhage, we should not attempt this operation without these appliances. I have found it useful to dip the inner end of the plug in a strong tincture or solution of perchloride of iron. The plug should remain for twenty-four hours, and if there be hemorrhage it should be repeated until that cease, after which the vagina should be well syringed once or twice a day.

Case 3.—I am tempted to add a third case, in which the removal of the polypus was attained by even simpler means. Some ten or twelve years ago I was summoned to see a lady from whom "something had fallen down;" it was found that the uterus was inverted. I found the lady in bed, and protruding from the vaginal orifice was a tumour certainly five inches long and about three in circumference. It was not the uterus, undoubtedly; it seemed, and I believe was, a fibroid tumour of very lax structure, as though the fibres had been separated from each other. When trying to make out its origin from the cervix uteri, I found that the structure yielded quite easily to my finger; without hemorrhage, and by a little manipulation, I separated the whole from the cervix, whence it originated, and removed it from the vagina. There was not a drop of blood escaped, and I contented myself with advising astringent injections. The lady recovered well.—*Obstetrical Journal, Dec.* 1877, *p.* 618.

73.—TREATMENT OF FIBROID TUMOURS OF THE UTERUS.

By Prof. Alex. Russell Simpson. (Introductory Address as President of the Edinburgh Obstetrical Society.)

I do not know that I could well address you on a more important subject than the management of Fibroid Tumours of the Uterus. It is not only that they are very common, so that as many as one in every five women have been supposed to be the subject of them. In my own ward journal I find out of 68 cases admitted during the last three years with disease of the

sexual system, 7 cases of fibroid tumour, or 10·3 per cent. It is not even that they are detrimental to health and dangerous to life; but it is because they exert such a baneful influence upon the reproductive powers of those who are affected by them. Out of sixteen married women, *e.g.*, who were the subjects of fibroid tumours, and in regard to whose reproductive functions I have made a note, I find that only three had borne children. Thirteen out of the sixteen were barren.

Their Pernicious Influence on Reproduction.—Other diseases of the generative organs may influence the reproductive process at one or more of its stages; but a fibroid tumour may mar the process at any or every step. It may compress the ovaries and hinder ovulation. I have known them such a cause of dyspareunia that the patient could not suffer marital intercourse; or, insemination having taken place, they may hinder the onward progress of the spermatozoa, so that the ovula are never fertilized. Or, again, the spermatozoa may have travelled through the irregular expanded cavity of the uterus, and fertilized an ovum in the Fallopian tube; but the fertilized ovum does not effect a lodgment in the uterine nest, or becomes attached only to be cast off as an early abortion or miscarriage. The fibroid tumour may lead thus to interruption of pregnancy at any stage, and where the development of the fœtus goes on to term, the child is apt to be found at the close in some irregular presentation or position in consequence of the irregularity in the walls of the cavity where it is lodged. Let us even suppose the pregnancy arrived at full time, and the fœtus fairly placed. Yet labour may be complicated in any of its stages. The first stage may be lingering; the second seriously obstructed; the third complicated with grave hemorrhage. They even carry danger on into the puerperium, for a fibroid in the walls of the uterus may be a source of post-partum hemorrhage, or may become the seat of an inflammatory process, dangerous in itself, and extending dangerously to surrounding structures.

Their Pathological Nature and Anatomical Seat.—I do not require to detain you with any lengthened exposition of their pathological nature and their anatomical relations. Enough if I remind you that whatever their ultimate relations, they spring from the middle muscular wall of the uterus, with the elements of which the neoplasmic elements are homologous, so that they present, as their most constant and characteristic constituent, quantities of unstriped muscular fibres, largely rudimentary, though sometimes more developed, collected in twisted bundles among a densely fibrous and granular connective tissue; and that, as regards their position in the walls, they may be found growing towards the serous surface (subperitoneal), or imbedded in the middle of the wall (intra-mural),

or projecting towards the uterine cavity (submucous); and this relation of them to the thickness of the uterine parietes is a matter not merely of anatomical interest, but of great clinical importance, both as regards the symptoms that arise and the line of treatment that may most hopefully be instituted.

Natural Terminations.—Let us glance for a little at the methods by which, in certain cases, the disappearance of these tumours is brought about under the efforts of nature.

1*st*. *Fatty Degeneration.*—There are few things more certain in pathology than that these myomata occasionally wither in consequence of a process of fatty degeneration being set up in their constituent fibres. In cases where a gravid uterus has such a tumour in its walls, after the expulsion of the ovum whether prematurely or at full term, when the usual process of retrograde metamorphosis is set up in its proper muscular layers, the same process is set up in the homologous neoplasm imbedded among them; and as the uterine walls return to their normal pre-gravid measurements, the tumour diminishes and may altogether disappear. Our vice-president, Dr. James Young, will recall such a case which I saw with him a few years ago. The patient had an easy enough labour, but a troublesome third stage and some degree of post-partum hemorrhage. When I saw her next day the uterus was felt with its fundus an inch or two below the level of the umbilicus, and a large firm equable mass could be easily manipulated through the relaxed abdominal wall, growing from the upper part of the anterior wall of the uterus, and reaching the size of a child's head into the right hypochondrium. This large fibroid mass became greatly reduced in size during the puerperal week, and when the patient passed from under Dr. Young's observation, it had diminished to the size of a hen's egg, and was lodged within the pelvic cavity. Two years ago I had under my own care and observation a primiparous lady, 36 years of age, in whom, from the fifth month and onwards, I had occasion from time to time to observe the presence of a fibroid tumour on the anterior aspect of the uterus. Through the abdominal walls it felt as if of the size of a walnut, and it projected very distinctly from the surface of the organ. Immediately after labour I could still feel it without any difficulty. During the puerperium it grew less with the lessening size of the uterus; and when the patient went to her home in the country, at the end of two months, I could not any longer trace its presence. I saw her some eight months later, in the fifth or sixth month of her second pregnancy, and still the outline of the uterus was smooth; the tumour had melted completely away, and had never been reproduced. A third instance I may adduce from the history of a patient of whom I shall have more to say by-and-by.

but who states that after a labour, when she was about thirty years of age, an irregular mass was detected by her medical attendant in connexion with the uterus, which she declares to have had the same consistence as a fibroid mass now growing from the same organ. That tumour diminished and disappeared during her convalescence, so that she ceased to be able to detect its presence.

That these tumours, which disappear in the walls of a puerperal uterus, melt down by a process of fatty degeneration and disintegration does not, to my mind, admit of a doubt. There is more room for discussion as to whether such a process occurs in them under other circumstances. The fibroid-invested uterus is an organ in which nutritional disturbance has already taken place, and is prone to be repeated. It is an organ, therefore, in which inflammation is apt to be set up, and in which the effects of chronic inflammatory processes in general, and this of fatty degeneration of structural elements in particular, are frequently to be observed. The new growth, of somewhat low vitality, as evidenced by its feeble vascularity, will be specially liable to such change. Hence, in the case of a patient who died under efforts made to relieve her of the symptoms resulting from a group of fibroids in the walls of the uterus, and in whom inflammatory action had been set up in the organ, I could see some of the unstriped muscular fibres taken from the fibroids containing fatty particles, and evidently in an early stage of fatty degeneration. More frequently I have seen such degeneration, and in a more marked degree, in sections of such tumours that had sloughed out or been expelled under strong uterine contractions. I see no reason, therefore, to doubt that a similar process of disintegration may go on in them more slowly but as effectually in cases where the inflammatory action does not run so high, or spread so widely, as to lead to the patient's death; or in which the tumour continues to be crushed up within the uterine parietes without being expelled through the genital canal. It could only be by a fortunate accident, as it were, that such a change could be seen under the microscope, for it is precisely in such a case that the patient passes from the care of the clinician to better hands than those of the pathological anatomist.

2nd. *Calcareous Degeneration.*—Another degenerative process to which they are occasionally liable is that which results in the diminution of their size and condensation of their structure along with a deposition of calcareous matter in their substance. This calcareous or osseous degeneration is most likely to occur in the fibroids of aged women. According to my experience, it is more frequent in the sub-peritoneal than the other varieties, though last session, through the kindness of Dr. Watson of

Mid Calder, I had the opportunity of exhibiting here an illustration of such a process affecting a sub-mucous fibroid, and to this preparation I shall refer more particularly anon.

3rd. *Pedunculation and Extrusion.*—A third process by which nature sometimes gets rid of these neoplasms results from the physiological properties of the matrix in which they are developed. For the walls of the uterus have inherent in them a tendency to undergo a remarkable degree of development whenever any body gets lodged within them; and when the muscular fibres have attained a certain degree of development, they manifest their physiological character by contractions sometimes tonic and continuous, at other times more energetic but intermittent. The effect of such contractions on a fibroid that is not purely intramural, is to force it towards the surface in the direction of least resistance. If they grow from the external plane of the muscular coat they are driven towards the serous surface so as to become more definitely subperitoneal. They may there finally become pediculated, and where the neck is narrow the mass may even become detached and be left free to move about in the peritoneal cavity. Some years ago I had in my ward a woman upwards of forty years of age, with such a roving body of the size of a hen's egg, that could be pushed about anywhere in the abdominal cavity; and the nature of it seemed to be certified by an absolutely similar body of the same firm consistence and slightly larger size, which still retained its attachment to the fundus uteri by a narrow pedicle. We are more familiar with those which are driven in the opposite direction. I do not know that there is anything to show or to lead us to suppose that the inner plane of the muscular fibres is a more frequent seat of their development. Probably it is rather owing to the circumstance that the tumours so situated are subjected to more forcible compression from being more completely embraced by the uterine parietes. But however this may be, it is far from uncommon to find them in the form of an intrauterine fibrous polypus, sessile or attached to the interior of the organ by a longer or shorter neck, undergoing thus a process of slow extrusion; and in certain cases we meet them driven down under more energetic action of the uterus, and thrown off by this process of spontaneous expulsion. In such instances the tumour is driven on, covered with an investment from the uterine mucosa. But sometimes the mucous membrane on the surface sloughs off or ulcerates through, and then the naked tumour is driven out of its bed and born by the process which has been termed spontaneous enucleation.

Their Treatment.—Let us turn now, if you please, to the consideration of the means we may adopt to imitate these natural processes or to expedite them where they are in progress.

If I do not dwell on the influence of food in this class of cases, and more particularly on the influence of a strict animal diet as suggested by Cutter, it is simply because I have not gathered any experience for myself in this line of treatment. For the same reason—that I wish rather to deal now with methods of treatment of the value of which I have been able to satisfy myself—I pass over the treatment of uterine myomes by means of electricity, and proceed at once to the consideration of their

Medicinal Treatment.—Here I may at once frankly say, that I know of no drug which on being introduced into the system finds its way to a uterine fibroid, and acts in the way of a solvent on its structures.

1*st.* *Mineral waters.*—At the same time, I cannot doubt the powerful influence for good exerted by some of the mineral waters, *e.g.* those of Kreuznach, the virtues of which were advocated here by Dr. Engelman last session. For I have seen patients who were suffering from such tumours in whom the symptoms were relieved, and in whom the growth of a previously increasing tumour was arrested, if the bulk was not immediately diminished. These mineral waters seem to me to exert some portion of their influence by acting as sedatives to the sexual organs, lessening the activity of the circulation in them, and so reducing the nutritional activity. One can understand how in this way the effects of the chronic inflammation going on in the organs may be removed and a check be given to the further increase of the neoplasm; how even, when the muscular walls of the uterus are disburdened of their inflammatory products they may quietly but continuously begin to take on their function, and contract so firmly around the growth as to favour its disintegration.

2*nd.* *Bromide of potassium and chloride of calcium.*—A similar influence, it is at least highly probable, may be exerted by the bromide of potassium. This drug, which enters largely into the composition of some of these waters, has certainly a powerful sedative influence upon the generative organs; and though, as I have said, neither it nor any other can be regarded as a simple solvent of uterine myomata, I have a strong impression of its value in modifying the conditions that favour their development. But I have long ceased to trust to it alone in their medicinal treatment. In some instances I have administered the chloride of calcium as recommended by Washington Atlee and Spencer Wells, but I have failed to meet a case where the progress of the tumour was sensibly affected by its use; and it is to be remembered that its prolonged administration is not a matter of indifference, as Wells has noticed the premature development of an arcus senilis in patients who were employing

it. One of its expected advantages, indeed, is the deposition of calcareous salts among the tissues of the tumour or in the walls of its nutrient artery; but there is no means of controlling their deposition in the desired site, whereas in the case of the bromide of potassium, we have to do with a salt which, however it may act, does not lodge in the system, but is being constantly eliminated, so that I have had patients taking it for many months and even for a year or two without its producing any constitutional effect, if only they were careful to attend to the recommendation to suspend its use during the menstrual week.

3rd. *Ergot of rye.*—But the drug that most powerfully and unmistakably affects the growth of fibroid tumours of the uterus is the ergot of rye. Its influence on the developed muscular fibres of the uterus naturally led to its employment in cases of fibroid tumours with hypertrophy of the surrounding walls; and the concurrent testimony of many gynecologists puts the action of ergot in the treatment of these growths among the best established phenomena of therapeutics. The preparation, for example, to which I have already referred as having been sent here for exhibition by Dr. Watson, was taken from the body of an unmarried female, æt. 52, whose case was brought under the notice of the London Obstetrical Society in 1871 by Dr. John Brunton. She had been the subject of a fibroid tumour which reached up as high as the umbilicus, but which disappeared in the course of six or seven months under the administration of full doses of ergot at each menstrual period. When the patient died last summer of disease altogether unconnected with the sexual system, and after she had ceased to suffer from any further hemorrhages, there was found in the upper part of the uterus and growing from the fundus and anterior wall—as some of you saw—a condensed and partially calcified fibroid of the size of a small mandarin orange. For many years I have been in the habit of treating certain cases of uterine fibroids with ergot of rye during the menstrual period, and bromide of potassium in the intervals, and in many instances with good results. Rather more than a year ago, there came to my ward in the Infirmary here, a woman whom I had had under my care ten years ago in Glasgow, suffering from a fibroid which caused the uterus to rise like a fourth-month organ above the pubis. Under the treatment I have indicated, the tumour began distinctly to diminish in size, and as the patient became freed from her distress, I lost sight of her till she came back bringing with her her old prescriptions. She stated that she had kept well for three or four years, when she began again to suffer from her old symptoms and the re-appearance of a tumour in the lower part of the abdomen. She had gone to live in a

distant part of the Highlands, and allowed it to progress till now it filled the abdomen and reached a handbreadth above the umbilicus. On this occasion I kept her under treatment for nearly three months, administering every second day a hypodermic injection of 2½ grains of ergotin, with the result that a slight but very appreciable diminution occurred in the mass. She was obliged to go home sooner than I desired, but she left under promise to return if the tumour began to grow larger, or she began to suffer from any aggravation of her symptoms.

The narration of this case has led me into the statement of the method that I have found to be the most effectual for obtaining the full benefit of the drug. For it seems to me that the doubts as to its efficacy are traceable to one or other of three different canses: 1*st*, The use of an inert preparation of ergot; 2*nd*, An imperfect administration of it; 3*rd*, An inappropriate condition of the patient. As to the *first*, it is one of the commonplaces of obstetrical therapeutics that quantities of inert preparations of ergot are in all the markets, and that much of the uncertainty as to the value of the drug is due to the employment of powders, extracts, and tinctures which are devoid of all active properties.

Secondly, with regard to the mode of administration: while almost any preparation of a good ergot will give the desired effect, it was a step of immense importance in the satisfactory treatment of uterine fibroids when Professor Hildebrandt, of Königsberg, demonstrated the safety and certainty with which an active dose of ergotin could be administered hypodermically. He showed what my own experience, as well as that of Byford, of Chicago, and others has amply confirmed, that the repeated subcutaneous injection of from 2 to 5 grains of ergotin can be counted on with great certainty to excite appreciable contractions in the walls of uteri in which the muscular fibres have become hypertrophied. The preparation of ergotin which I have found most satisfactory is the same which I brought under the notice of this Society in treating of the complete evacuation of the uterus after abortions.

℞. Ergotinæ, ʒ ij; aquæ, ʒ vj; chloral-hydratis, ʒ ss. M.

Twelve drops of the solution or rather mixture—because the ergotin is partly dissolved and partly suspended—gives a dose of three grains, and this may be regarded as a medium dose, to be administered daily, or every second day, or twice a week after the influence of the drug begins to be manifested.

In making this hypodermic injection, it is necessary to take care, 1st, that the fluid carry with it no small globules of air; and, 2nd, that the point of the syringe be carried deeply down through the skin and areolar tissue, right into the muscular strata. Sometimes the injection may be made in the abdominal

walls; in most cases they are borne best in the gluteal regions. I cannot understand how the practice has crept into our hospitals, but I observe that when students are called to make such an injection they pinch up the skin and push the point of the needle obliquely through, and occasionally to some distance among the cellular tissue beneath the skin. Now the pinching up of the skin may do good and serve to make the surface somewhat tense; but the needle should certainly always be carried in as perpendicularly to the surface as possible, and straight down with one quick stroke into the muscular tissues. Such a preparation so introduced, is not liable to be attended with the suppurations which have deterred some practitioners from the continuance of this mode of administration. It is but rarely even that the patients complain of the pain. I can only recall two out of the many patients in whom I have used it, who objected to the frequent repetition of the injection on the score of the local suffering. They may be made daily, or every second day, for several weeks; or after some frequent injections for a month they may be continued once or twice a week for many months without producing any constitutional disturbance.

Cardiac disease does not constitute a contra-indication to its employment. At least one patient, to whose case I have already referred as having got rid of some fibroid mass during a puerperium, was commended to my care chiefly because of the distress she suffered from a cardiac affection. I found, indeed, that she had well-marked valvular disease of the heart; but it seemed to me that her distress was largely due to the presence of a group of fibroid tumours, some of which were subperitoneal, but one at least was intramural or submucous, and associated with pretty free menstrual discharge. The subcutaneous injections were freely carried out in this case without the faintest drawback, and with the result of a lessening of the menorrhagia and a diminution in the bulk of the lower portion of the general mass, along with great improvement in the patient's general health. Again, I have used the ergotin hypodermically in the case of a patient who is the subject of chronic asthma and bronchitis with nothing but satisfactory results.

But there remains the *third* point as to the appropriate cases. I believe Hildebrandt has correctly indicated the condition most favourable for the use of ergotin in stating that the tumour must be intramural or submucous; in other words, it must be surrounded by layers of muscular fibre, sufficiently developed to be capable of being excited to contraction, and sufficiently powerful to exert some degree of pressure upon the body in their embrace. For the beneficial action of the drug in such

cases depends upon its property of stirring and keeping up continuous contraction in the unstriped muscular fibres of the uterus, the effect of which is in some cases to push the compressed tumour more rapidly towards the uterine orifice, and so to favour its extrusion from the cavity; in others, so to interfere with its nutrition that it ceases to grow or even begins to wither, probably in consequence of fatty degeneration being set up in its fibres. The ergotin perhaps exerts a secondary influence in the direction of cutting off the nutritive supply of these bodies, by tending to cause contraction in the walls of the uterine arteries, and so lessening their calibre. Such an influence is not to be denied, and although it is altogether subsidiary towards the elimination of these fibroid tumours, it makes the employment of the drug very serviceable for the alleviation of one of their commonest symptoms, viz., the excessive losses of blood.

Sometimes, I have said, the hypodermic injection leads to the rapid extrusion of the tumour. This occurred in the case of a poor woman whom I saw with Dr. Balfour, of Portobello, and who came into my ward to be treated. She had long suffered from pelvic pains and uterine hemorrhages, and had got into the habit of taking opium freely and frequently. The fundus uteri reached more than half way between pubes and umbilicus, and the sound passed four inches into the uterine cavity. There was a degree of tympanitis and abdominal tenderness that necessitated the administration of chloroform to enable us to make a satisfactory examination. Under the use of the hypodermic injections of ergotin the discharge at first diminished, then a fetid discharge began to escape. Frequent examinations even with the finger pained the patient, who was a virgin, æt. 48; but after two months' almost daily use of the injections a sloughy mass was found to have been expelled into the vaginal canal, which was detached from the uterus by means of the écraseur. It was so soft and pulpy that I thought at first the diagnosis as to the nature of the tumour must have been incorrect; but on more careful examination it was found to be a fibro-myome in a gangrenous condition, many of its fibres breaking down, and some of them crowded with fatty particles. The patient recovered health to a great degree, notwithstanding that she had an attack of pleurisy before she left the Infirmary, and the tympanitic state of the abdomen never disappeared.

In other cases where the tumour has not been expelled, it has become reduced in size. Two years ago I saw, with Dr. Cullen, of Airdrie, an unmarried lady, 35 years of age, who had become very anæmic from excessive losses of blood. She had an intramural fibroid of the size of a child's head. The hypodermic injections were carried out at somewhat lengthened intervals,

but kept up for many months by a sister, as they lived at some distance from their doctor; and when the patient came to see me last spring, her discharges were less profuse, her tumour much reduced in size, and her general health greatly improved. I might make almost the same remarks regarding a very similar case which I saw some nine months ago with Dr. Peter Stewart, of Glasgow; only the tumour in this case was originally larger and more irregular, and wedged more firmly into the pelvis, and the diminution in size is not yet so pronounced. In a note which I had the other day from Dr. Stewart, he tells me he is using the ergotin in another case of fibroids with promising results.

In certain cases, the beneficial action begins to make itself sensible to the patient by relief of her symptoms before there is any distinctly appreciable reduction in size of the mass. Moderation of the often exhausting hemorrhages is a frequent observation. But sometimes other symptoms thus soon get relief. A lady, for example, who was sent to me by Dr. Leith, of Comrie, in addition to the weakness caused by menorrhagia, was suffering from symptoms of pressure on the bladder. The uterus, occupied by a fibroid tumour, was about the size of a large fist, movable, and with a patulous cervix. I administered an ergotin injection, and advised its repetition two or three times a week. In six weeks the patient returned, having experienced great relief from her pressure symptoms, and gathering strength as she had had less loss of blood. Still I could not satisfy myself that the uterus felt much lessened in bulk. After the continued use of the injections, made chiefly by the patient's husband for two months longer, the diminution in the size of the uterus was very perceptible, and the patient's general condition was still improving.

In two instances of unmarried ladies where the tumours were subperitoneal, but where there was considerable enlargement of the uterus, and a degree of menorrhagia that would itself have constituted an indication for the use of ergot, I have observed that under the ergotin injections the tumours, without sensibly decreasing in size, became, in the course, in one, of three, in the other, of five months, more superficial.

Lastly, I have noticed that in several instances, where the tumour was of large size and where the ergotin-injection treatment has been instituted, a growth, which up till that time had been steadily increasing in size, has had such an arrest laid upon it that it ceased to enlarge. Two ladies, both unmarried, whose history is illustrative of this occurrence, are at this moment in my mind. In one, I had the opportunity of witnessing and watching the slow but steady growth of a fibroid during several years, despite the use of the Kreuznach waters,

bromide of potassium, chloride of calcium, and internal administration of ergot. Since, about eighteen or twenty months ago, however, she had a series of ergotin injections carried out for some two months, there has been no increase in her girth, though her general health is better, and her limbs are stouter. In that case, the period of life may have favourably influenced the condition, as she ceased to menstruate a few months after she had begun this treatment. But in the other case, the patient is still only 35, and though menstruation goes on regularly the flow is less, and there has been no increase in her abdominal measurements since more than a year ago she first began to use ergotin injections.

I might multiply the histories of patients suffering from fibroid tumours of the uterus, whom I have seen benefited by the adoption of Hildebrandt's treatment; but I fear I have already taxed your patience. In view of the doubts that are still expressed in some quarters, however, I feel that it is quite worth while to have adduced these illustrative cases, which I hope will help to convince you, as they have convinced me, that the hypodermic injection of ergotin is a therapeutic agency of the first importance in the treatment of fibroid tumours of the uterus; and that where we find such a tumour causing much hemorrhage, seated in a uterus with a patulous cervical canal, and surrounded by some layers of well-developed muscular fibres, we may have recourse to its employment with a well-grounded expectation of seeing the symptoms relieved and the tumour greatly reduced in size, or it may be expelled altogether, or at least brought more speedily within the sphere of

Surgical treatment.—If I may now ask your attention to the operative measures to be employed for the removal of fibroids, I shall again leave out of consideration some important proceedings, such as the producing of a slough, and so procuring the disintegration of the growths, as proposed and practised by Dr. Greenhalgh, with the actual cautery, and the extirpation of the uterus or portions of it through an opening made in the abdominal walls—an operation which has now been pretty frequently performed, which I have myself witnessed at the hands of two different operators, and which doubtless has a triumphant future before it. I wish to speak rather, and that very briefly, of the operations that will always be applicable to an immensely larger number of cases, where we have to do with tumours not so imposing in their size, but important from the frequency of their occurrence, and the urgent call they often make on operative skill.

1*st*, *Torsion.*—The first method that I always think of employing for the removal of an intra-uterine fibroid to which I have got access, is the simple twisting of it so as to loosen it from its

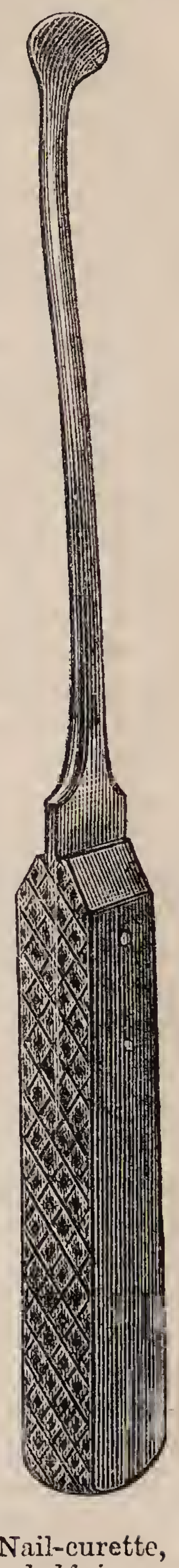
attachments. Whether it be pediculated or sessile, large or small, as soon as it can be firmly grasped with the fingers or with a vulsellum or pair of abortion forceps, it should be twisted steadily and firmly round and round in the same direction until its attachments are felt to yield. It is astonishing sometimes with how friable a neck even a large fibroid still hangs to the uterus, and with how loose a base a broadly sessile one may be imbedded in the wall. The bleeding that takes place in such cases is but little, and soon comes to an end. I have seen a fibromyomatous tumour passing from the uterus into the vaginal canal of such large size as to baffle all attempts to reach its base or pedicle even when the four fingers of the hand were pressed into the cavity, and yet the pedicle gave way when the great mass had been rotated several times in one direction. A considerable gush of blood took place at the moment that it was felt to be set free, but no further escape took place during the two hours and more that I spent in breaking down and extracting the growth through the narrow outlet.

2*nd*, *Cutting*, *Crushing*, *or Scratching*.—When the pedicle or base will not yield under torsion, they must be divided by some cutting, crushing, or scratching instrument. The cases are now pretty numerous in which I have removed such bodies with the polyptome. For its satisfactory employment, however, the tumour must have a distinct and rather narrow neck, and in any case it is apt to cut obliquely and may leave a fragment behind. This, of course, usually atrophies; still one would rather have a smooth surface left. In one or two cases where the pedicle was more easily accessible from below I have divided it with scissors.

In several, where I apprehended hemorrhage, I have used the écraseur, and always take the instrument to any case of removal of a fibroid. It is not always easy of application, however, and occasionally when it is at work dividing a fibroid of firm texture, it disappoints you by giving way.

I fancy most gynecologists who have had much to do with such cases have sometimes felt the want of an instrument for working their way through the pedicle or base of these bodies. So one reads in the histories of the use of the point of a bistoury, the blade of which is wrapped in lint; or the handle of a scalpel or such like. I see from the number of the American Journal of Obstetrics that has just come to hand, that Professor Gaillard Thomas showed to the New York Obstetrical Society an instrument which he uses for cutting through the base of sessile fibroids. It resembles a spoon on a long handle not unlike one of Simon's curettes, only the edges, instead of being simply sharp, are toothed or serrated, so as to more easily cut through the tissues, and it seems well adapted for its object.

In removing these bodies, I have sometimes been able to make my way deeply through the base with the nail of the forefinger alone. But there always meet one some strands of fibre that are too tough to be thus torn through, and besides the nail bathed in the blood begins to soften, till one wishes that it were made for a little of steel. After trying curettes of various kinds, I have come for the last eighteen months or so to make use of the instrument which I now show you, and which I find immensely serviceable in the digging out of fibroids and other new growths in the uterus. The flat sharp point serves as a substitute for the nail, the edge of which it resembles in size; the slightly curved stem is long enough to be easily carried up to the fundus uteri, and so slender as not to occupy much space in the canal; the handle is broad and square, to give a good hold and purchase in the working of it, and roughened on all sides except on the surface towards which the sharp edge of the instrument is directed. When it is being used, the tumour is sometimes dragged down with a volsellum by an assistant, particularly when it is very mobile. At other times I have used it where the uterus was simply steadied by pressure from above the pubes. The point is guided to the root of the tumour by the index finger of the right hand, which closely follows it in its track through the severed tissues, the handle being worked all the while by the left hand. It is thus an instrument of extreme simplicity and as safe in its working as the nail of the finger, for which it is a substitute.


Nail-curette, *half-size.*

One of the patients, in whom I first employed it, had a history which presents several points of unusual interest. She was a widow lady, 60 years of age, who brought me a letter from Dr. Mackenzie, of Kelso, stating that she was the subject of a pelvic tumour, that she had been operated on by Sir James Simpson ten years previously for some uterine affection, probably a polypus, that she had at that period been for some time in a lunatic asylum, and that again she was becoming the subject of mental derangement. The mental malady was clear enough. The old lady was under the constant apprehension that something was to happen to her, and she would pass an hour at a time in screaming out. But she attributed all the distress

herself to the swelling, which was very perceptible in the lower part of the abdomen, reaching up to within two inches of the umbilicus. This was found to be the uterus enlarged with what I took to be, from its firm consistence, the vascular bruit, and other characteristics, an intra-uterine fibroid. She was extremely anxious to have the tumour cut out. This I did not feel warranted in undertaking to do; but as the case seemed a fair one for the hypodermic injections of ergotin, I commenced their use every second day. During six weeks that I had her under treatment here, I could perceive that the drug was exerting an influence; the tumour began to sink in the abdomen; the os uteri began to expand; her mental condition improved slightly, and she went home. For some weeks, by some mistake, atropin injections were administered instead of ergotin. By-and-by, the mental symptoms getting more urgent, she was put under the care of Dr. Tuke, in the Saughton Asylum, where again I had the opportunity of watching the progress of the case. Dr. Tuke's assistant, Dr. Bower, carried out the injections very carefully for a few weeks till the expansion of the os and the descent of the tumour had so far progressed as to render the interior of the uterus easily accessible to the exploring fingers. Partly from the deterioration of the system from imperfect nourishment,—for she made a difficulty about her feeding,—and still more from the drain that for some months had been taking place from the uterus, the patient had become very feeble, so much so that Dr. Mackenzie had great doubts as to the propriety of attempting the removal of the growth. It was attached to the fundus and posterior wall of the uterus over a widely extended surface; and I was disappointed to find that it was not simply imbedded and encapsuled, but closely incorporated at some parts with the muscular coat. It was out of the question to apply an écraseur, the mass could not be twisted out, and I do not know how I could have got it detached from its place, but for the help of the nail-like curette. By having it dragged upon with a strong volsellum by my friend Dr. Horatio R. Storer, of Boston, who was present, and by patient determined scratching through of all resistent textures, it was severed from the uterus and removed. Seeing that there was such an intimate union of it with the uterine walls, I was not surprised to find that the mass, though mainly myomatous in structure, presented at some points sarcomatous elements. The patient rallied from the operation and recovered her strength to a very remarkable degree. As I had anticipated, however, from the anatomical structure of the tissues, the tumour was reproduced. From time to time sloughy masses were expelled, which presented a purely sarcomatous character, and some twelve months after the date of the operation the patient died

under an excessive loss of blood.—*Edinburgh Medical Journal, Jan.* 1878, *p.* 580.

74.—DIFFICULTY IN DIAGNOSIS DUE TO ROTATION OF OVARIAN TUMOURS.

By Dr. J. Knowsley Thornton, Surgeon to the Samaritan Free Hospital for Women and Children.

The fact that the differential diagnosis of an abdominal tumour may be rendered exceedingly difficult or impossible by the tumour being an ovarian one with a twisted pedicle, seems to have escaped the notice of the many writers on ovarian disease. The two following cases illustrate very well the difficulty of giving, not only a correct diagnosis, but also a correct prognosis in such cases :—

On Dec. 18th, 1876, E. A. B., aged twenty-four, single, came under my care at the Samaritan Hospital, during the absence of Mr. Spencer Wells, with the following history and condition :—

She was taken to see Mr. Spencer Wells on Nov. 10th, 1877, by Mr. Stevens, of Hoddesdon, Herts, with a moderate-sized abdominal tumour. After a careful examination Mr. Wells expressed doubt as to the nature of the case, and requested to see her again in six weeks.

Mr. Stevens had first been called to see her in consequence of a sudden attack of pain in the right side just before her menstrual period. This first attack had occurred six months before she came under my care, and each returning period had brought a recurrence of the pain, which was sometimes so severe as to make her roll on the floor in agony. After the visit to Mr. Wells, Mr. Stevens wrote, and described another seizure starting three days before the period and continuing for seven days, being only partially relieved by subcutaneous injections of morphia and a grain of opium every four hours. The pains on this occasion were so like commencing labour that Mr. Stevens examined per vaginam, and found a long virginal cervix, and the uterus unaffected by the paroxysms.

When she came under my observation she was a healthy looking girl, with full colour and no emaciation. She measured $30\frac{1}{2}$ inches round the abdomen at the umbilical level, $4\frac{1}{2}$ inches from the ensiform cartilage to the umbilicus, and 7 inches from the umbilicus to the pubes. There was a small moveable tumour occupying the right side of the abdomen and reaching to the umbilicus, and slightly across to the left of the linea alba. Its borders were overlapped by intestine all round. It could be felt to the right and in front of the uterus per vaginam, and appeared closely connected with it. The os and cervix

were natural; the uterine cavity measured nearly $3\frac{1}{2}$ inches. I could not feel certain as to fluctuation in the tumour, but I thought it did fluctuate. There was also a slight wave of ascitic fluid.

I kept her under observation during a period, but did not gain any fresh light as to the nature of the case. The pain was much less severe than at the former periods. Ascitic fluid now began to accumulate rapidly, and the patient, who had been in very good health, lost her appetite, got a yellow look, and complained of general malaise. I therefore determined to make an exploratory incision, and remove the tumour if it were found possible to do so. The period ceased on Jan. 20th, and on Jan. 24th, assisted by my colleagues, Dr. Bantock and Dr. Champneys) the latter administering bichloride of methylene), I made the incision usual in ovariotomy to four inches. Six or seven pints of brownish ascitic fluid escaped on opening the peritoneum, and the tumour presented at the opening. It was of a dark liver colour, and had a firm sodden feeling, but evidently contained fluid. Some filmy adhesions to the intestines on the right side were ligatured with fine silk and divided. Firm adhesions to the appendix vermiformis, requiring three ligatures, were then dealt with. The tumour was tapped, and about a pint of thick tar-like fluid evacuated. The substance of the cyst was very friable, and broke away from the claws of the trocar. It was seized with Nélaton's forceps, the opening enlarged with scissors, and some inner cysts, with similar contents, broken up, and the tumour withdrawn. I then found a short, hard, and twisted pedicle on the right side of the uterus; it was untwisted, transfixed, and tied in two halves, and the tumour cut away. No trace of vessels could be seen in the pedicle, and I believe it might have been cut without hemorrhage. The left ovary was found enlarged, and forming a grape-like bunch of small pediculated cysts. Its pedicle was secured in the same manner as the other, and it was removed. All the ligatures were of fine silk, and they were all cut off close to the knots and returned into the peritoneum, which was then thoroughly sponged out and the wound closed by fine silk sutures. The tumour weighed 1lb. 12oz., and there were seven pints of the mixed ascitic and cystic fluids. It was an ordinary multilocular one, and some of the small cysts which had not been broken contained pure dark fluid blood. A large patch of the main cyst wall was very thin, apparently from ulceration of a portion of the lining membrane, and so soft that the finger was easily pushed through it.

The patient made an excellent recovery, the highest temperature and pulse being noted exactly twenty-four hours after the operation—99°·8 and 104 respectively. The bowels acted after

enema on the ninth day, and on the thirteenth day she was moved down into the convalescent ward, and on the twenty-sixth day after operation went to a convalescent home.

Slight metrostaxis came on five days after the operation, and continued four days.

I have heard of and from the patient frequently since; she is very stout and strong, but suffers at times from hot flushes and headache. Menstruation is irregular; but does occur from time to time with relief to the above symptoms.

The second case is similar but more perfect as an example of the subject, because I saw her before the twisting of the pedicle occurred in the first instance, or, at any rate, before any marked symptoms of the rotation were apparent.

M. M., aged thirty, single, came to see me in August, 1877, from Woolland in Dorsetshire. She had been under the care of Mr. Tarzewell, of Sturminster Newton, who had diagnosed an ovarian tumour.

I found a small ovarian cyst with very free fluctuation, and apparently free from any adhesions. By pelvic examination I could only just detect the tumour. She was a healthy-looking young woman with a full habit, and apparently not suffering in any way from the tumour, except that it prevented her stooping, and doing various things which her occupation of cook rendered necessary. The Samaritan Hospital was just closing for its autumn cleaning, and I was leaving town, and therefore advised her to come up again in October and have the tumour removed. On her return in October she was thinner, and not looking so well, and her skin had a yellowish hue.

History.—In August, 1876, first noticed a lump in the right side, about the size of a hen's egg; it gradually enlarged, and in May, 1877, she began to suffer pain in the bowels, was feverish, and was in bed for two weeks. From this time the tumour gave her more trouble; she often had pains in the abdomen and back, and suffered more than formerly at the menstrual periods. On her return home in August all her pains greatly increased, and from that time till she returned in October she suffered from constant pain in the back, with bearing down of the uterus, and stooping became impossible from the pain it caused in the abdomen.

On proceeding to examine her I was at once struck with the fact that though she had lost flesh and was, consequently, generally smaller, the tumour itself had evidently decreased in size. Proceeding to feel it I found it firmer, and no longer so distinctly fluctuant; indeed, I was in doubt whether there was any fluctuation, it had to me much more the characters of a soft fibroid. Pelvic examination revealed no change, except that the tumour seemed much more closely connected with the

uterus than I had thought. I was now inclined to regard it as a pediculated outgrowth from the uterus, but with my former impression fresh and strong in my mind was much puzzled. I asked Mr. Wells to see her with me, and he agreed that it was a doubtful case, and advised keeping her under observation. Others among my colleagues, who kindly saw her with me, declined to give a positive opinion; but the general leaning was evidently in favour of its being uterine—this view being encouraged by the general appearance of the patient, and the strong pigmentation of the linea alba.

I frankly acknowledged my doubts to her and kept her under observation till December 11th, when I decided, after fully explaining the nature of the operation, to make a small exploratory incision. She was both willing and anxious that I should do this as she was quite unable to earn her living, and I felt fully justified in proposing it, looking at the history of the case since it had been under my direct observation. I should mention that the tumour did not seem to have decidedly increased or decreased since October, but if there was any change it was in the latter direction. It appeared also to be as freely movable as ever in the abdomen.

On December 13th Mr. Meredith gave bichloride of methylene, and, assisted by my colleagues, Dr. Bantock and Mr. Doran, I made a small incision in the usual situation. The hemorrhage from the parietes was freer than I have ever seen it, and I was some time before I could open the peritoneum; when I did so I at once saw the familiar white, glistening surface of an ovarian tumour, and enlarged my incision to five inches; when more fully exposed the surface of the cyst was seen to have a mottled look, and, greatly to my surprise, was adherent in all directions to the parietes by firm films and bands which were very vascular; similar adhesions connected it with the omentum and intestines—many of these required ligature. The tumour felt so solid that I attempted to remove it whole, but finding this impossible without unduly lengthening the incision, I tapped it and let out a quantity of thick, dark grumous material; drawing out the empty cyst I found a firm, fibrous, twisted pedicle close to the right side of the uterus. I transfixed and ligatured it in two halves with medium silk; when I cut away the cyst the vessels were seen to be plugged with partially decolorised clot. The other ovary was plump and healthy, the uterus normal, but pulled somewhat over to the right, and somewhat out of shape. The very free hemorrhage in cutting through the parietes was explained by the fact that the chief blood supply of the tumour was received through its parietal adhesions. It is very remarkable how very freely the tumour could be moved about in the abdomen when we consider the extent of the adhesions. A

good deal of sponging of peritoneum was required, and I should have put in a glass drainage tube, but having performed the operation under the carbolised steam spray, and in every detail with the strictest antiseptic precautions, I did not think it necessary, feeling sure that any blood or serum which oozed after the peritoneum was closed would soon be reabsorbed. The tumour resembled very closely the one I have already described, except that it was not so completely dead; its vitality having been better maintained by the extensive vascular adhesions. A very large portion of the cyst wall was thinned and softened as in the former tumour, and I wonder it did not give way during my efforts to extract it whole.

The peculiar mottled appearance of the external surface of the cyst was similar to that I have noted in a case described by me in the twenty-seventh volume of the "Transactions of the Pathological Society," though not so marked as in that case, where the cyst was entirely dead and gangrenous.

The patient made a good recovery, but at 11 p.m. on the night of the operation her temperature rose to 101°·4, and the ice-water cap was put on, a precaution I have rarely found necessary since I commenced antiseptic ovariotomy. She was troubled with sickness for two days, and was a very difficult patient to feed; the only things she would eat at any time being boiled rice, green vegetables, and a little fish.

All the sutures were removed under the spray on the seventh day, two days later than usual, owing to my having to go out of town; the wound healed entirely by first intention. After the third day the temperature was never over 99°·0, and usually normal pulse, 72 to 80. Bowels acted naturally for first time on thirteenth day. I did not think it necessary to have them opened by enema as she took so little food. She got up on the same day, and went to the convalescent home on the twentieth day after the operation. These cases teach their own lesson so well that it may seem unnecessary for me to point out the features of special interest, and I will therefore only briefly summarise them.

The difficulty in each was to decide whether one had to deal with an ovarian tumour or some other. In both cases the uterine outgrowth seemed to me most probable as an alternative, extra-uterine fœtation being also suggested in the first case. In both cases the pain at the periods referred chiefly to the side on which the tumour was situated and to the back, was a marked symptom, though much more severe in the first, and this also pointed to uterine rather than ovarian disease.

In both, the twisting of the pedicle shortened the connexion between the tumour and uterus, and increased the likeness to a pediculated outgrowth.

In both, the stoppage of blood supply, though in different degree, led to decrease in size, with corresponding solidification of the tumours.

In both, extreme congestion, with large extravasation of blood into the tumours, seems to have preceded the complete obstruction to the circulation. The reason for this, as I have pointed out in the paper in the Pathological Transactions already referred to, is to be found in the thin walls and large size of the veins with the thick muscular coats of the arteries in ovarian tumours. Doubtless the increase of pain at the periods was due to the increased vascularity with proportionate congestion. In both a certain jaundiced appearance was observed, due, as I believe, to the reabsorption of the colouring matter of the blood. I have seen it more than once in patients with large hæmatoceles which have been left to nature.

In the first case the whole tumour was practically dead and non-vascular, only small portions of its peritoneal coat near the adhesions showing any trace of vascularity, and it affords us a beautiful example of the harmless nature of dead tissue, provided it is guarded from all sources of external contamination—an example accentuated for those who will carefully study the subject by a comparison with the course of events in the case I have already alluded to, where strangulation of the tumour took place after the causes of putrefaction had been introduced into the tumour from without. I have met with another case which illustrates this, and those who are interested in this side of the subject of rotation of ovarian tumours will find some remarks on it in a paper published by myself in the Medical Times and Gazette, July 28, 1877, in which brief notes of the first of these cases are also given. It only remains to point out that though we know, on the evidence of *post-mortem* observations, that ovarian tumours have withered and become harmless from their blood supply having been cut off, we know nothing of the history of these cases; and looking to the condition of the tumours in my cases as seen after removal, I think they were in both happily removed when they were. Had either of them from gradually softening or from some sudden external agency, ruptured and poured its contents into the peritoneum, the results would have been almost certainly disastrous. Such a termination might have occurred had one regarded them as fibroid outgrowths and given a favourable prognosis. In similar cases, I believe, the wise course is to make an exploratory incision, as it is impossible, without its aid, to give either a correct diagnosis or prognosis. I am much more inclined to advocate this course now I have tested the value of the antiseptic system in abdominal section. I believe, if carefully and strictly performed by Professor Lister's method, very little,

if any, danger attaches to an exploratory incision, *provided* no previous puncture or tapping has been made to introduce causes of putrefaction into the cyst.—*Obstetrical Journal, Feb.* 1878, *p.* 720.

75.—THE SILK LIGATURE AS A METHOD OF SECURING THE OVARIAN PEDICLE.

By Dr. J. Knowsley Thornton, Surgeon to the Samaritan Free Hospital for Women and Children.

Soon after commencing to perform ovariotomy, I determined to give the silk ligature a fair trial in a series of consecutive cases. It seemed to me worthy of such a trial, because of its universal applicability, its cleanness and neatness, both at the time of operation and in the after-progress of the case, and because it yielded such good results in the hands of those who only used it in cases in which it alone was applicable.

With the works of Spencer Wells and Peaslee in our hands, in which the whole question of its use is ably discussed, a paper on this subject may seem unnecessary. It differs, however, from the treatment of the subject in either of the works named, inasmuch as it deals only with one of the methods of using the silk ligature which they describe, viz., that in which the ligature or ligatures are made to transfix the pedicle, both ends being then cut off close to the knot and returned with the pedicle stump free into the peritoneal cavity. I should hope that the old plan of leaving one end of the ligature hanging out at the lower angle of the wound has for ever passed away. It has no advantages to recommend it, and at least two serious disadvantages to contraindicate it. It necessitates the cutting off of the distal end of the stump by ulceration, and, during this slow process, it keeps open a moist highway for the causes of irritation and putrefaction to travel by from the exterior of the wound to its deepest recesses; to the very parts, in fact, which we wish to heal as quickly as possible.

Before proceeding to discuss the details of its application and the difficulties and dangers to be encountered, I will sketch briefly the various methods by which the healing process is accomplished and the ligature disposed of.

It always seemed to me probable that, though the tissues included in the ligature were tightly enough constricted to prevent hemorrhage, they were not usually so grasped as to completely destroy the nutrition of the distal end of the stump. The following observation confirms the correctness of this view. In one of my cases, I tied a large piece of omentum adhering to the cyst in two places and divided it between the ligatures, thus leaving a stump on the outside of the tumour very like that of

the ovarian pedicle. I afterwards injected the cyst with Beale's Prussian blue fluid, and, when the injection was complete, noticed that the distal end of this stump was blue, though not so blue as the proximal part, and there was no appreciable escape of the fluid from its cut surface. Microscopic examination showed that the injection had passed into the minute capillaries, but not into the larger vessels, except where it had passed into them by the anastomoses beyond the ligature.

If the injection would so pass, the blood would also pass; and in this way the vitality of the part would be at any rate partially sustained until the other processes by which the distal end of the stump is nourished had time to be completed.

The five following conditions of the stump after ligature I have myself observed in the dead or living subject. I do not claim any novelty for the observations, but, so far as I am aware, they have not as yet been all recorded side by side.

1. The ligature is buried in the peritoneal coat of the pedicle, the portions on the opposite sides adhere, and vascular connections are thus rapidly established. Cells penetrate between the fibres of the silk and break up the ligature, which is absorbed and disappears. The finer the silk used, and the softer the pedicle, the more likely is this to happen.

2. Lymph is thrown out over the ligature and cut end of the stump; in this new vessels are formed, and the same result is obtained, but more slowly.

3. The stump adheres to some neighbouring peritoneal surface, and from that new vessels pass into it, through which its nourishment is obtained.

It will at once be seen how the passage of some nutriment through the capillaries in the constricted portion must aid either of these processes, and we must also remember that the pedicle is surrounded by the nutritive fluid naturally present between the peritoneal surfaces, and probably always somewhat increased when repair is going on.

The first two of these methods are the most favourable, and are those which we must aim at securing. As a result of either of them, the stump may be in a few months merely represented by a little button of smooth tissue at the site of the pedicle. Such a result I had an opportunity of seeing when assisting my colleague, Dr. Bantock, at an exploratory operation a few months after ovariotomy. The exploration revealed the above condition of the pedicle, which was not adherent to any neighbouring surface, and was also quite free from the malignant disease of the peritoneum which had caused reaccumulation of fluid. How often such a result is obtained it is of course impossible to say, but I think we may fairly suppose it to have occurred when there is no symptom of any irritation about the

pedicle, or at any rate in the majority of such cases (*vide* Cases 1, 7, 8, 10, 11, 14, 15, 17, 18, 22, &c.). When the stump adheres to some neighbouring peritoneal surface, most commonly some part of the intestine, because they are generally in contact with it, we are by no means likely to get such a good result. We may have no sign of mischief for some time, usually not till after the bowels have been moved. Sometimes, however, some intestinal irritation shows itself much earlier, especially if there be trouble with flatulent distension; indeed, the latter is often a sign of the irritation. When symptoms follow the first action of the bowels, they usually appear from the tenth to the fourteenth day. There is usually some rise of pulse and temperature; this may be considerable, and is, I think, proportionate to the extent of adhesion which has been disturbed. There is often depression of spirits, pain about the pedicle, and sometimes hardness may be detected in the iliac fossa, or by vaginal or rectal examination. All the symptoms may gradually subside, or they may persist for some time with obstruction to the passage of the flatus and fæces or with troublesome diarrhœa and tenesmus. In the former case, fatal obstruction may result; in the latter, the case will end in discharge of pus by rectum or vagina, and all will go well. In the cases given below, examples of each of these results may be found except that of fatal intestinal obstruction.

4. Fatal hemorrhage may follow the use of the ligature, and the same accident has been reported to me by several surgeons as having occurred in their practice. This most formidable accident is due to the escape of one or more of the large veins, which are common at the outer edge of the pedicle (pampiniform plexus), from the external loop of the ligature. It requires great care and some experience to avoid it, when treating a very broad and short pedicle.

5. The ligatures may remain uncovered around the pedicle in cases where diffuse peritonitis with effusion of serum or rapid septicæmia follows the operation; and if the patient survive only a few hours, they will be found just as they were when applied; or if she survive for a longer period, thay may be found somewhat loose, with an ulcerated groove round the pedicle, the distal end being in a sloughing condition.

I do not think the fatal result is much influenced by the ligature in this last class of cases, though it is *possible* that it may become early soaked with the products of putrefaction and be a constant source for their propagation; whereas the tissues might have been able to dispose of the putrid or irritative matter had no such absorbent material been present to hold it. Excluding these cases from our consideration, we still have a formidable array of dangers to consider; but, fortunately,

experience teaches us that the favourable terminations are the most common after the use of the ligature; as the cases here given will show. There being only five fatal cases out of a total of thirty-eight, and only one of these fairly to be attributed to the ligature; two of the others (Cases 2 and 19), being hopeless under any treatment, and the other two sufficiently serious to render it impossible to attribute the fatal result to the ligature, at any rate with certainty.

I will endeavour to point out certain precautions to be adopted in using the ligature, which will, I think, ensure for it results even more favourable than my own have been up to the present time.

First, as to the material. It should be the pure Chinese silk, without any admixture of cotton. The hemp ligature still finds some advocates, but it has, so far as I can see, no advantages, and the great disadvantage of being a vegetable instead of an animal tissue; and it is slippery and cutting to the fingers when wet. One advantage claimed for it is that it shrinks after tying, and hence is a security against hemorrhage. From experiments I have made with it, I do not believe it shrinks at all after it is once thoroughly wet, and this it always is before it is tied; and, if it did so shrink, I should not consider it an advantage in view of what I have already said as to the partial circulation through the constricted portion. The silk, if properly tied, never fails to *sufficiently* constrict. As to the method of applying the silk, I believe it to be important that the two ligatures should interlock when tied, so as to form a figure of eight, otherwise they may pull apart at the point of puncture and hemorrhage may result. In a thin pedicle, it is easy to avoid puncturing a vessel, and the interlocking of the ligatures is a matter of less consequence; but, in a thick one, it is often impossible to see and avoid every vessel of sufficient size to be dangerous if punctured. If more than one transfixion be required, the ligatures should all interlock so as to form a chain. This has one disadvantage, tending, in a broad short pedicle, to cause a drag upon the outer loop of the ligature-chain, and hence to increase the risk of hemorrhage from slipping of some of the large veins to which I have referred. The weight of the uterus and other ovary, aided perhaps by the contractile nature of the tissue of the broad ligament, increases this risk. It is well, therefore, in every case, to examine the pedicle-stump carefully just before closing the incision; and, if any sign of hemorrhage or slipping be found, to apply a fine silk or catgut ligature by transfixion on the proximal side of the other ligatures, this last ligature to include the veins already referred to. I always adopt this plan now, using either fine silk or catgut wherever the outer edge of the broad ligament feels tight as

one passes the fingers along it from the pelvic brim to the edge of the pedicle. In all the earlier cases, I employed very thick silk, and seldom more than a single transfixion, with an accessory ligature if necessary. Thinking this thick silk might be the chief cause of the trouble with the pedicle, I tried a medium thickness, tying the pedicle in smaller pieces and with more transfixions. I found the results satisfactory, and I now use the finest silk that will bear the necessary strain in tying.

Since performing ovariotomy antiseptically, I have, whenever it was possible, sewn the posterior peritoneal cut surface of the pedicle over on to the anterior surface of the broad ligament with fine silk or catgut with a view of covering over the raw surface, and at the same time placing it in immediate contact with a peritoneal surface, from which it might receive its blood supply quickly. This method has also the great advantage of carrying the stump over to the anterior surface of the broad ligament, and so away from the risk of adhering to the intestines. I am not prepared to advocate this proceeding, however, unless the operation be performed antiseptically, because it has the disadvantage of opening up the cellular tissue of the broad ligament, and it is very probable in some of the cases small vessels are also transfixed in their continuity. In cases where the pedicle is very broad, thick, and short, this sort of flap operation would not be possible; but I think a suggestion made to me by Mr. Lister, would answer well in such cases, viz., to unite the cut peritoneal edges of the stump with catgut sutures, placing a few strands of catgut between the sutures to drain, and prevent any possible risk from accumulation of serous fluid and tension.

I am aware that the catgut ligature has been successfully used instead of silk for securing the pedicle, but the results with the silk are so good that I shall not change it for catgut, at any rate, until one can be more certain of always getting a good sample of the catgut. As at present sold, it is too uncertain in its quality.—*Brit. Med. Jour., Jan.* 26, 1878, *p.* 125.

76.—OVARIOTOMY AT THE SAMARITAN HOSPITAL.

By T. S. Wells, Esq., Consulting Surgeon to the Hospital.

In the course of some clinical remarks on ovariotomy made at the Samaritan Hospital, Mr. Spencer Wells detailed the great improvements that had taken place during the last few years in the diagnosis and treatment of ovarian tumours. The operation is now conducted in a light, airy and quiet room, in which the patient remains alone with her nurse for at least a week after the operation. No visitor is admitted to witness the operation without declaring that he has attended no *post mortem*

nor any case of infectious disease for a week previously. The patient is placed upon a table, lying on her back, warmly clothed, the lower limbs covered with a blanket, the head and shoulders supported by pillows, the knees and hands secured by straps, a perforated india-rubber sheet so applied that only the front of the abdomen is uncovered, and she is asleep under the influence of what Mr, Wells believes to be the safest and best of known anæsthetics, bi-chloride of methylene. All the instruments that can be wanted for the most complicated case are ready and at hand. There must not be, he says, any threading of needles at the last moment. The nurses have a precise number of perfectly pure and soft sponges, and plenty of small fine linen cloths for use before the sponges are wanted. Supposing daylight direct or reflected fails, Mr. Wells has tried various kinds of reflecting lamps when searching for vessels deep in the pelvis; but the most useful of all is the small medical lamp recently introduced by Colin, of Paris. All this is ready before visitors come into the room, and they are then requested to observe the most absolute silence. It is quite a common thing for an operation to be completed without a single word having been spoken by the surgeon, the assistants, or the nurses; and if a remark is made by an unwary visitor, it is at once hushed. The incision in the abdominal wall, the stopping of bleeding from superficial vessels by torsion forceps, the division of the peritoneum, the exposure and tapping of the cyst, the separation of adhesions, the management of adhering omentum or intestine, the breaking down of inner septa, and the withdrawal of the tumour from the abdominal cavity, the treatment of the pedicle, the examination of the opposite ovary and uterus, the thorough cleansing of the pelvic and peritoneal cavities, the use of drainage-tubes if required, the closure of the wound, the dressing and bandage,—all are matters of detail of great importance. The patient is carried from the operation table and placed in a warm dry bed. The room is at once cleared and darkened, and when she awakes she finds herself alone with her nurse. Recent changes in the management of the patient after operation have been chiefly in the direction of regulating temperature. Enough opium is given to relieve pain, but not more. The patient is kept warm enough to encourage free action of the skin without being made uncomfortably hot. Food and drink are regulated by the instinctive desire for them. All the nurses are instructed in the use of the thermometer, and they are directed whenever the temperature rises above 100° Fahr. to keep the head cool by means of the ice-water cap. If the skin is dry, very small doses of aconite are given frequently (half a drop of the tincture every half-hour). It is only in the rare exceptional cases of septicæmia, septic peri-

tonitis, or pyæmic fever that large doses of quinine or of salicylate of soda are thought of. In some few cases bleeding from the arm has been necessary, but as a rule the patients are let alone after the operation and they get well.—*Practitioner, Feb.* 1878, *p.* 127.

77.—COMPLETE INTRA-PERITONEAL LIGATURE OF THE PEDICLE IN OVARIOTOMY.

By Alban Doran, Esq., F.R.C.S., St. Bartholomew's Hospital.

When the pedicle of an ovarian tumour is too short to allow the use of the clamp, it is now the practice among the most experienced operators to transfix it with several ligatures, to cut short the ends of those ligatures, to return the pedicle within the abdominal cavity, and to close the wound. This practice may conveniently be termed "complete intra-peritoneal ligature," for the sake of brevity, and in contradistinction to the system, now obsolete, of leaving the extremities of the threads uncut and dependent from the external wound.

"Nothing but the large amount of success which has recently followed the adoption of this most unsurgical method appears to me to justify it. Its advantages as regards the probabilities of peritonitis and the risk of hemorrhage from the slipping of the ligature (it being part of the plan to cut the peduncle close to the ligature) are evident." These are the words of a well-known hospital surgeon, written within the last fifteen years, and still retained in a popular text-book. Yet complete intra-peritoneal ligature is now justified, not only by the success of the bold earlier ovariotomists, but also by a precise knowledge of the changes produced in the stump of the ligatured pedicle, changes which are now known to consist, not in gangrene or diffused peritonitis, once considered inevitable, but in processes which are harmless from the first, and ultimately beneficial.

These processes I have myself had the opportunity of observing in a case where death occurred a very few days after operation. But before describing the specimen of ligatured pedicle which I examined in this particular instance, it may be interesting to consider the history of the complete intra-peritoneal ligature as performed by the earlier operators. These surgeons had not the advantage of the scientific sanction which that practice has gained from the now well-known experiments of Spiegelberg and Waldeyer on the effects of the ligature when applied to the cornua of the uterus in animals. Before those investigations were made public the practice must be considered almost, one may say, empirical. Much valuable

information on the subject is scattered among the pages of certain special works, particularly the standard productions of Mr. Spencer Wells and Dr. Peaslee. In order, however, to discuss the matter with precision, it is very advisable to refer to the original records of the early ovariotomists.

Dr. M'Dowell, admitted by Mr. Wells to be "the first rational ovariotomist," did not adopt the complete intra-peritoneal ligature. His practice was to leave the ligatures uncut and hanging from the lower end of the incision in the abdominal walls.

But Dr. Nathan Smith of Connecticut, the second American ovariotomist, in his first operation in 1821, not only ligatured two arteries in the omentum with strips of leather from a kid-glove, but also tied two arteries in the pedicle. This is in accordance with principles recognised by the most experienced modern operators; ligature of the pedicle, as a whole, being hazardous, since the single thread is apt to slip. The ends of all the ligatures were cut short, and the external wound closed, the stump of the pedicle having been returned into the abdominal cavity. Dr. Smith, then, was the first to adopt the complete intra-peritoneal ligature. The patient recovered.

In 1829 Dr. David Rogers of New York ligatured separately several large vessels in the pedicle of an ovarian cyst, and returned the stump of the pedicle with the ligatures cut short. The operation was perfectly successful. In 1835 Dr. Billinger adopted the same proceeding with satisfactory results.

Lizars's first *bona-fide* ovariotomy was successful, but the ligature encircling the pedicle was "carefully left out" of the external incision. In relating the next case, which ended fatally from peritonitis, he says: "I now gave this enormous mass to my assistant, Mr. Macrae, passed a ligature round the pedicle, and tied it firmly, and then cut close to the tumour, securing three open-mouthed vessels of the pedicle....... I now stitched up the wound, carefully avoiding the intestines and omentum."

The ligatures of the pedicle, or, more strictly speaking, of the vessels of the pedicle, were evidently not left uncut and dependent from the wound, for the careful Scotch surgeon would surely have recorded the fact had it been so, as in the preceding case. Thus Lizars's second ovariotomy was the first instance of complete intra-peritoneal ligature ever performed in the British Isles. "The peritoneum investing the parietes which adhered to the tumour, and also those portions of this membrane investing the colon and small intestines which adhered to the tumour, were of a bluish-black appearance, and tore with ease under the fingers, being evidently gangrenous." After expressing his belief that the patient should have been bled the night

after operation, Lizars remarks: "This probably would not have saved her, *for such contusion was inflicted* that the stamina of life were apparently not capable to stand such a shock and repair the evil." There is, then, clear evidence that death was due to the bruising of the peritoneum, and the fatal result of this operation throws no discredit on the treatment of the pedicle.

Lizars's third operation was incomplete, and throws no light on the subject of this paper, nor need we refer to Dr. Granville's cases. But Jeaffreson, of Framlingham, in 1836, ligatured an ovarian pedicle as a whole, and cut short the ends of the thread. This case was perfectly successful. The history of Mr. Phillips's ovariotomy, performed in 1840, is very instructive. The stump of the pedicle was tied as a whole, and returned into the abdomen with the ligatures cut short. The patient died on the fourth day. "Upon examining the Fallopian tube, the ligature was found in its place; but it was evident that, from its hypertrophied condition, it resisted the necessary constriction (although Mr. Samwell had used much force), and the extravasation was a consequence of oozing from the extremity of the tube. *That oozing, however, had long ceased, for nature had blocked up the vessels.*" Thus wrote Mr. Phillips in the "London Medical Gazette," October, 1840. A few ounces of undecomposed blood were found in the peritoneum. Dr. Peaslee, in quoting this case, appears to infer that death was caused by the loosening of the ligature, or, as Phillips would express it, by the resistance of the pedicle to necessary constriction. But on reading the case itself as recorded by the operator, it will be found that the patient was suffering from choleraic diarrhœa the day before operation, unknown to her medical attendants. In fact, she died with severe choleraic symptoms; moreover, the above quotation shows that the hemorrhage had been slight, and was checked by natural processes.

Thus complete intra-peritoneal ligature was frequently adopted with success, or, at the worst, without being the cause of fatal results, in these early cases of ovariotomy, when the surgeon acted at an enormous risk, without anæsthetics, and without taking precautions now known to be essential by the light of greater experience. Moreover, such necessary hygienic measures as washing out the peritoneum with antiseptics was not ventured upon. In short, I think it is clear, from the above records, that the practice in question was justified by clinical experience on the human subject before the experiments of the German physiologists.

These experiments showed the changes that actually take place in intra-peritoneal ligature of the stumps of excised portions of the horns of the uterus in bitches. A communication

between the distal and proximal parts of the stump is established by inflammatory plastic effusion, and the ligature is unravelled by granulation-cells insinuating themselves between its fibres.

Dr. Tyler Smith appears to have been the first authority who regularly and systematically advocated complete intra-peritoneal ligature. Recently it has been adopted in hundreds of successful cases where the pedicle has been found too short for the clamp to be safely applied. Ligatures of bleeding vessels in the omentum are also cut short. Mr. Spencer Wells informs me that, on one occasion, he left as many as forty ligatures in the abdominal cavity without any evil effects. In the more perilous operations for the removal of solid growths of the uterus, complete intra-peritoneal ligature may also be practised with impunity; this is proved by Mr. Knowsley Thornton's case, recorded in the "Medical Times and Gazette," April, 1877. That gentleman strongly advocates the use of the silk ligature, even in cases where the clamp has hitherto been thought advisable.

An important section of contemporary medical literature furnishes us with a strong proof that complete intra-peritoneal ligature of the ovarian pedicle is firmly established. The student finds it advocated in those educational works on the theory and practice of his profession which he studies when preparing for examination, and retains, or ought to retain, in his library for consultation and reference in after-life.

Mr. Erichsen approves of this mode of ligature, at the same time epitomising its history as follows:—

"Some of the earlier American ovariotomists, especially D. L. Rogers, cut the ligatures short, and returned the stump of the pedicle into the wound. This practice was revived by Tyler Smith, in 1861, and has been adopted by him, by T. Bryant, and others, with the happiest results, the ligatures either becoming encapsuled or being discharged after a time through a suppurating track. It appears to me that if the ligature be used, this is the best method to be adopted; and if practised with carbolised ligatures, it would probably be most successful."

Mr. Holmes states, still more decidedly: "When the clamp cannot be fixed on the pedicle of the tumour, on account of its proximity to the uterus, without injudicious traction on that organ, the best plan is to perforate the pedicle with a needle threaded with stout wire, and tie it in halves, the ends of the ligature having been flattened down so as not to irritate the neighbouring parts, and after cutting away the tumour down to within about half an inch from the ligature, drop the pedicle back into the belly. In a case treated successfully in this way,

I searched some time afterwards carefully for the wire by palpation from the abdominal wall and from the vagina, but could elicit no sensation of its presence."

In the second edition of his "Practice of Surgery," Mr. Bryant makes some interesting observations on the history and on the advisability of complete intra-peritoneal ligature. He describes the pathological appearances noted in the pedicle in one case of his own, and in another recorded by Dr. Peaslee in a Transatlantic medical serial. A reference to Mr. Bryant's work will show that the intervals between the operation and the death of the patients in these two cases are similar to those to which I am about to refer. This surgeon concludes his disquisition on the subject by asserting that "with short and broad pedicles, in which the vessels are usually small, the cautery may be employed, or the pedicle ligatured in two or more parts with whip-cord, the ends of the ligatures cut off and dropped in, and the wound afterwards closed."

The precise pathological changes produced in the ovarian pedicle by complete intra-peritoneal ligature have been well displayed in the two following cases, one of which has been under my own observation, and has not hitherto been recorded.

In 1872 Dr. Bantock exhibited before the Obstetrical Society the stump of an ovarian pedicle from a patient who died of cancer one year after double ovariotomy had been performed upon her. The hempen ligature applied, with its ends cut short, to one of the pedicles, was found on dissection to have been completely absorbed excepting its knot, which remained as a hard body the size of a hemp-seed, covered by peritoneum. The bulging of the tissues over each side of the groove formed by the ligature had brought the strangulated portion of the stump at once into close contact with the unstrangulated proximal part. Through the slight irritation produced at first by the pressure of the ligature, the proximal part had thrown out plastic lymph, which had conveyed nutritive plasma and also capillaries to the distal portion of the stump, and thus saved it from gangrene. In a case like this, the stump ultimately atrophies, for reasons evident to any surgeon with a superficial knowledge of pathology. As for the ligature, it is destroyed in the manner demonstrated by the experiments of Speigelberg and Waldeyer.

In the summer of this year I made a *post-mortem* examination of a patient, aged thirty-seven, who died in the Samaritan Hospital from septicæmia, on the sixth day after the removal of a large multilocular ovarian cyst. The pedicle had been treated by complete intra-peritoneal ligature, and there was no evidence that death was in any way due to this method of treatment.

The stump of the pedicle was an inch broad, and its inner border was about a quarter of an inch from the fundus of the uterus. It was not in a sloughy condition, nor was it even congested. It was separated from the appendages of the uterus by four silk ligatures, none of which had produced ulceration, but all were covered with bands of lymph bridging over the constriction they had produced in the tissues which they encircled. A more interesting feature remains to be noticed. The outer extremity of the distal side of the pedicle was already very firmly united to the broad ligament by well-organised lymph. This plastic effusion must have been exuded some days before death, prior to the onset of septicæmia, and therefore very shortly after the operation. The lymph covering the ligatures was evidently more recent; it also showed signs of breaking down, its plasticity being diminished by the constitutional disorder which had proved fatal to the patient.

This early and intimate adhesion of the distal part of the stump of the pedicle to the broad ligament is represented in a lithograph, which I have shown in St. Bartholomew's Hospital Reports, for the production of which I am indebted to Mr. C. Berjeau. It might be suggested that the adhesion referred to existed before operation, the ligature being passed under it, Such, however, was not the case, as I carefully observed the pedicle and the process of ligature when Dr. Bantock removed the ovarian tumour. The specimen may be seen in the Museum of the Royal College of Surgeons, Pathological Series, No. 1642 C.

Thus the surgeon need no longer dread any evil effects when he thinks it desirable to leave ligatures enclosed in the abdominal cavity after a serious and complicated operation. Hence small foreign bodies do not necessarily produce disastrous consequences, even when impacted in a wounded structure in the neighbourhood of delicate organs irritated by disease, and by unavoidable surgical manipulations.

From the success of the complete intra-peritoneal method of ligature in ovariotomy we may deduce the important corollary, that necessity may justify the setting aside of surgical principles previously considered as laws never to be violated with impunity. Such deviation from precedent may yet be applied with advantage to other operations.

Note.—Since sending the above to the press, Dr. Bantock has drawn my attention to another specimen, showing the effects of complete intra-peritoneal ligature on an ovarian pedicle. Hempen thread had been employed, on the theory that that material shrinks when moistened, and thus tightens its hold on any structure around which it may be tied.

This specimen is from a girl eighteen years of age. On 5th

April, 1876, Dr. Bantock removed a large multilocular tumour, weighing seven pounds, from her left ovary. The patient made a good recovery, but in the following October the right ovary became the seat of a sarcoma. She was readmitted into the Samaritan Hospital, where she had undergone the first operation, and the abdominal cavity was opened, but on account of the character of the tumour it was not deemed advisable to remove it. The patient died on November 3, 1876.—*St. Bartholomew's Hospital Reports, vol.* xiii., 1877, *p.* 196.

78.—CLINICAL OBSERVATIONS ON OVARIOTOMY.

By Dr. J. Thorburn, Professor of Obstetric Medicine in Owens College; Obstetric Physician to the Manchester Royal Infirmary.

The ordinary steps of the operation of ovariotomy you will find in every recent text-book; you have had leisure to study them, and two very good opportunities of seeing them. Judging, moreover, from the present state of our out-patient *clinique*, there will be no lack of similar opportunities in the future. Instead, therefore, of recapitulating these details, I will confine myself to one or two points which I have been trying to think out for myself, as far as my opportunities have permitted.

1. I have come to the conclusion that the operation of ovariotomy is beginning to be far too rashly undertaken, or, at any rate, in too early a stage of the disease. When ovariotomy was a new operation, those who practised it were often accused of selecting their cases, *i.e.*, of operating only on those cases which appeared likely to be good ones, and refusing to operate on those which, although they might possibly save a patient from inevitable and almost immediate death, were more likely, by their issue, to discredit the operation and the operator. If Sam Slick's statement that there is a good deal of human nature in man be true, these accusations were probably not quite unfounded. But, as the operation has been more largely studied and practised, and especially as it has been gradually found out that the unpromising cases, *i.e.*, the cases of women past middle life, reduced, not in size, but in apparent vigour, by former tappings, with a peritoneum accustomed to the presence of tumours and trocars, are by far the most favourable ones, this kind of selection is becoming a thing of the past. But another, and, in my opinion, as great a danger is arising. Everybody is now supposed to be capable of performing ovariotomy, and the *éclat* of a successful case among the patients and friends of the operator is so great, that such cases are eagerly sought after, and a curious page of medical history

would be furnished by a collection, if such a thing were possible, of the results of ovariotomy during the last ten years in purely private practice. The splendid lists of successful operations which have been published by our great English and American ovariotomists are not withont danger also to younger men who aspire to follow in their wake. During the past twelve months, I have tapped three healthy and otherwise vigorous women for ovarian tumour, and in each case have counselled considerable delay before having recourse to ovariotomy; in all these cases, the urgency of the friends has led to other advice, to operation, and to death. I have just heard also, within the last few days, of the case of a young lady who, after ailing for some time, was found to have ovarian tumour. Immediately she was taken to London for a consultation, operation was decided on, and, in less than three weeks, she was diagnosed, consulted over, operated on, and buried. Let me give you my very decided opinion that ovariotomy should not be so rapidly and rashly undertaken. I would advise you, in every case, to try one tapping at least; it will help in diagnosis, and it *may* cure. I have cured one case in this way which I thought was a growing multilocular cyst. If your patient be comparatively young, and otherwise in fair condition, try a second tapping at least, and carefully ascertain the rate of growth. If, after this, you are satisfied that life is beginning to be jeopardised by the exhaustion of tapping, by wearing pain, or otherwise, then ovariotomy may be performed with hopefulness and with greater probability of another addition to the splendid *rôle* of increased years of human life referred to with justifiable pride by Mr. Spencer Wells at the late meeting of the British Medical Association.

2. Another point to which I may allude, in connection with the operation, is the size of incision advisable. The question of a long or a short incision has been much discussed formerly, and statistics have been drawn up as to their relative value. Such statistics have, in my opinion, no importance whatsoever. The larger incisions were made, as a rule, for larger or more adherent tumours, or for cases more difficult in other ways; hence the short incisions were pretty sure to have the best of it. I cannot see myself what difference an inch or two more can make in the danger of an incision of the kind, and I have seen, both in my own practice and in that of others, great danger from adhesions which might have been much better treated with a larger wound, and from dragging at, instead of enlarging, the opening. Even in the case of unilocular cysts, there are often adhesions which require a good opening to treat them safely. I would advise you, then, in almost every case, to make an incision of fully four to five inches, and not to hesitate to enlarge it at once as freely as seems necessary to command a good view of what you are doing.

3. The treatment of the pedicle is a subject which has from the first been much discussed. You will find in Mr. Wells's work a most temperate and thorough discussion of the various methods that have been used, including its retention outside the peritoneum by the clamp, and its treatment within the peritoneum by ligatures, *écraseur*, acupressure, cautery, &c.; and his vast experience has led him to adopt the clamp for the great majority of his cases. I am bound to say, however, that the use of a strong well tied silk ligature, cut short and allowed to fall into the abdominal cavity, has so far commended itself to me. The late Dr. Tyler Smith had a very fine series of cases treated in this way; it was this which induced me to try the plan, and it is so superior in handiness, and in every way, to my mind, so much more simple, that I fancy I am likely to continue its use until I have met with a case where I find inconvenience from the after-presence of the ligature, or until I have been unfortunate enough to meet with one where a ligature of my own tying has proved insufficient to prevent hemorrhage. I have unfortunately seen a very promising case lost in this institution from the slipping of the pedicle out of the clamp, which certainly looked as if nothing could have escaped from it. Bear in mind, however, that very powerful and pure silk, *i.e.*, animal tissue, well carbolised, should be used. This must perforate the stump of the pedicle, so that it cannot slip off at the end, and must then be tied in two separate portions. If the pedicle be very thick or broad, three or even four portions must be tied separately. If these precautions be observed, sufficient force may be used in tying to render any danger from shrinking an impossibility. A touch with the galvano-cautery might add to the apparent safety where the operator is timid; the ordinary cautery is apt to leave behind it some *débris*, which is at any rate unnecessary.

4. The influence of a hospital atmosphere for evil upon cases of ovariotomy has been so clearly proved that it can hardly admit of any doubt; and possibly the Infirmary Board, when it has overcome some of its more pressing difficulties, may be induced to provide us with a separate building or thoroughly detached ward for such cases alone. It is not, therefore, with the view of disputing the dangers incurred from hospitalism that I point out how, nevertheless, with reasonable care, much may be done to obviate these dangers. We are now in a building which is acknowledged on all hands to be, in many sanitary respects, as bad as it can be, and where erysipelas has frequently been peculiarly rife. During the short time that I have been connected with it, there have been ten cases of ovariotomy: three by Mr. Southam, two by Mr. Heath, and five by myself; of these, eight have recovered with hardly a bad symptom,

and there has been no septicæmic mischief. One, already referred to, died from the slipping of a clamp, and one (my own) from secondary hemorrhage from numerous pelvic adhesions.

5. Antiseptic treatment, in its relation to ovariotomy, is a subject full of immediate interest. The intimate connection between all forms of septicæmia and certain germs, and the destructive effect of carbolic acid and other agents upon the latter, so ably advocated by Mr. Lister, seems to have passed into the region of absolute fact. Though not yet convinced, even by the exhaustive researches of my friend Dr. William Roberts, or by those of my old Edinburgh Infirmary colleague, Mr. Lister, that there may not be some *tertium quid*, chemical, electric, or otherwise, which may stand in the relation of cause, or at any rate, of promoter, both of sepsis and of germ-growth, the practice dependent on the theory that the one is the cause of the other seems so consonant with all present available hypothesis, and so approved by clinical facts, that I consider it utterly unjustifiable to deny our patients its fullest advantages. I will not enter into the question as to whether, in ordinary surgery, the processes of dressing advocated by some are not so complicated and unnecessarily troublesome as to deter many from the use of antiseptics—*ne sutor ultra crepidam*—but, in ovariotomy, I am sure that simpler and more effective, *i.e.*, more antiseptic, methods will suffice. The plan I have adopted in twenty-five successive cases of ovariotomy has been this: after tying the pedicle, I thoroughly smear it with carbolic glycerine and drop it into the cavity of the abdomen. This imparts a strong carbolic atmosphere to the peritoneal cavity, thereby increasing the effect of carbolised sponges, ligatures, &c. The well known effect of glycerine upon the uterus, which you have seen in our ward practice, is, no doubt, exerted also on the pedicle and adjacent parts; a good deal of their redundant moisture is absorbed, and the natural process of shrinking is hastened. In these last two cases, I have also used the carbolic spray apparatus, and my friend Mr. Lund has kindly directed its application. After passing a sufficient number of silver sutures across the abdominal wound, I next smear every interstice of it with the same carbolic glycerine, not forgetting its free application to the peritoneal edges. I then tighten and twist the sutures, and apply a piece of lint dipped in carbolic glycerine. Some long adhesive straps, a clean napkin, and a binder complete the process.

How or when the wound heals, I cannot exactly tell you; for I do not look at it for ten or twelve days. In the case of Hughes, it was first looked at on the eleventh day; the stitches were taken out as free from discharge as if they had been imbedded in a turnip or other vegetable body. To-day, we will

open up the dressings of Hoggard, and you will find the same result. All that is required is to remember that the cicatrix consists of very new tissue; and that you must provide, by adhesive straps and binder, for many weeks against the giving way of this new tissue; indeed, I consider such precautions more than usually necessary, seeing that you have, while hastening the adhesive process, diminished some of the inflammatory action which is usually supposed to make adhesion more complete. It is hard to say how far this real antiseptic treatment has modified the mortality of these twenty-five cases; but I will give you the results, premising only that, in every case, I have followed out the rules I have tried to impress upon you. I have not operated on a single case which did not appear to have a certainty of death within two, or at the most three, years; and in two of them death was imminent from peritonitis within a few hours. Six of these twenty-five cases have died—one from hemorrhage—I might almost say on the operating-table—two from peritonitis, which existed at the time of operation (in one of these I operated by gas-light; the symptoms were so urgent); one from hemorrhage from adhesions in the pelvis, about forty-eight hours after operation; one from shock on the night of the operation; and one from pneumonia coming on on the fourth day after operation, after the patient had twice managed to escape from her bed in the search for stimulants. The most vivid imagination could not possibly trace the existence of septicæmia in any one of them.

If the clamp were used, this mode of procedure would not be quite so easy; but I think it would be quite available if the whole were well saturated with the carbolic glycerine, and if, as suggested by Dr. Marion Sims, some cotton-wool were placed above it, to prevent, by its filtering properties, the entrance of germs from outside.—*Brit. Med. Jour., Jan.* 5, 1878, *p.* 5.

79.—ON A NEW MODE OF TREATING CERTAIN CASES OF RETROFLEXION OF UNIMPREGNATED UTERUS.

By Dr. JAMES BRAITHWAITE, Vice-President of the Obstetrical Society of London; Lecturer on Diseases of Women and Children at the Leeds School of Medicine; Surgeon to the Hospital for Women and Children.

We are all familiar with the treatment of retroflexion of the unimpregnated uterus by means of Hodge's pessary, and this, with the occasional use of the sound and treatment of the congestion, inflammation, or subinvolution, as the case may be, suffices for the rectification of the malposition in most cases, and for relief of much of the attendant distress in all. When, however, these means fail, and they sometimes do, owing to the

power on the long arm of the lever being unequal to the support of the weight on the short arm, we have only the use of the internal stem to fall back upon. This little instrument has always appeared to me unsuitable for the intended purpose. First, it is inefficient, owing to its being straight; secondly, and as a direct consequence, it is too long about its work, and during the lengthy period of its residence in the womb it acts like any other foreign body, keeping up enlargement and hyperæmia.

In precisely the cases in which the stem would be used, I have employed the plan of treatment which is the object of this paper to explain. If you want to straighten a crooked stick the usual way is to bend it in the opposite direction, and it seems to me the womb should be treated just on the same principle, provided there are no adhesions and no pain is caused by so doing. The uterus ought to be anteflected, and in this consists the principle of the treatment I propose to lay before you, combined with dilatation of the cervix and lower part of the body of the womb, for the double purpose of admitting the instrument and destroying the resistance of the tissues to the change of position.

Laminaria tents are not suitable for cases of retroflexion, they cannot, in fact, be passed, and some little practice is required to pass a sponge tent. The best tent for this purpose is a long, fine, and very hard one, without any wax coating. This is to be passed rapidly up to the point of flexion, and there held for a second or two, when it will absorb a little moisture, and becoming somewhat flexible will then take the required bend and pass into the uterine cavity. If this cannot be accomplished, a shorter and thicker tent should be passed up to the point of flexion, and on its withdrawal next day a longer one can be introduced without any difficulty through the os internum. On the removal of the tents it is well to wash the parts out with a little weak iodine lotion. The next step is to take a piece of brass wire, about eight or nine inches in length, and no thicker than can be easily bent to a right angle with the first three fingers and thumb of the right hand. The wire should be ready prepared as follows—one end should be covered with india-rubber tubing to a length half an inch less than that of the uterine cavity as ascertained by the sound. This must be very securely and neatly closed at the end, the end of the wire being enclosed but not included. The tubing should fit the wire pretty closely, and it should be firmly secured to it by strong hemp ligature close to its proximal end. At this end of the tubing is to be placed a button, fixed to the wire, not to the tubing. This, which is the uterine end of the instrument, is to be now bent into a curve somewhat like that of the uterine

sound, the point of greatest curvature coinciding with the os internum when the instrument is introduced into the uterus, which is the next step.

The flexion of the uterus must then be rectified, just as is commonly done by the sound. An inflatable rubber ball pessary, having a small central tube, is now to be threaded on to the wire projecting from the vagina and passed well up to the os, in order to retain the anteflector in the uterus by its upward pressure against the button when it is inflated with air. Only one thing now remains to be done, namely, to bend the portion of wire which projects from the vagina backwards, at a right angle between the buttocks, so as to lie closely in the sulcus between them. The position is now this: the uterus is anteflected—not simply straightened—the instrument cannot escape from the uterus, nor can it by any possibility enter further into it, even if the patient were to get up and sit on a hard seat, nor can it rotate so as to allow of the womb resuming its original position. At the same time there is no fixed point, and it accommodates itself to every movement of the body, such as coughing, sneezing, &c. The uterus is bent so as to stretch the part previously compressed by the flexion, but no harm results from this, for the tissues have become extremely pliable from the previous use of the sponge tent. No pain of any consequence results, but it is well to give a grain of opium, as a precautionary measure and also to confine the bowels. After four days, during which period the patient should be kept quiet in bed, the instrument is to be removed and the vagina washed out with Condy's fluid and water. A Hodge must be at once inserted; the uterus will now retain its position, but it is large and soft, and treatment must be directed to this. The patient should still be kept in bed, and a large dose of ergot given once daily, and vaginal injections of cold water used occasionally for the first three days. I think that to get the proper action of ergot we require to give a large dose seldom, rather than a smaller dose more frequently. I believe also that the uterus is more capable of contraction after a temporary enlargement, such as is induced by the treatment described, than it was before such treatment had been commenced, or than it would be had the treatment been more prolonged, as when a straight stem is kept *in sitû* for months. I have only treated four cases in the way described during four years, because it is alone extreme cases which require to be so dealt with. I was always successful in rectifying the malposition, but this did not in all entirely relieve the pelvic pains and other symptoms complained of.

I do not think anything would be gained by giving these cases in detail, and indeed they are less instructive than other

two cases in which I attempted the treatment but did not carry it out. In the first of these two there was a slight degree of chronic metritis, as evidenced by tenderness on pressure. In this case I was compelled to withdraw the instrument in a few hours, and give opiates to relieve the pain. As the dilatation by the tent caused much pain, I shall in future accept this as evidence that it would be better not to proceed further. It may be said that the metritis should have been previously treated, and this was done, but with very imperfect success, owing to the extreme flexion. I, however, afterwards kept this patient recumbent for some months, and succeeded in getting the malposition rectified by the use of a powerful Hodge. In the remaining case, either owing to want of care on my part or some misunderstanding on the part of the patient, I inserted the tent on the very day the menses were due. In the night she had regularly recurring and rather severe pains, ending in the expulsion of the tent. I kept her in bed during the remainder of this menstrual period, and then, on examination, found to my surprise that the uterus was quite in natural position. I inserted a Hodge at once as a precautionary measure. This case naturally suggests the possibility of making the uterus rectify a flexion by calling into play its own efforts, and it shows that the unimpregnated organ possesses active contractile power, for the pains were described by my patient as being at the last both severe and expulsive.

Dr. Braithwaite, in reply to several objections, said that he wished to be distinctly understood that the treatment described by him was only to be followed as a most exceptional thing in which the internal stem would otherwise have had to be resorted to. In answer to Dr. Routh, he disclaimed any idea of curing a bad case of retroflexion in four days, but claimed that after this treatment, as a fact, the uterus does remain in its natural position, provided a Hodge is at once inserted, and the other directions followed. Thus, the cure is probably not completed for several weeks. All the four cases treated were of moderate duration only, but were characterised by the uterus being large and heavy, so that a Hodge assumed at once a faulty position. Dr. Braithwaite quite agreed with Dr. Barnes' remarks as to the rarity of failure with a Hodge alone, combined with treatment for congestion and with rest, but claimed that it was in precisely those rare cases of failure that his plan of treatment was applicable. It succeeded because the steps by which the affection occurs are retraced in their proper order. The misplacement happens when the uterus is soft and pliable, and by the treatment recommended the same state is produced prior to rectification. It seemed probable to Dr. Braithwaite that a less period than four days might succeed, and this would render the

necessary confinement less with some. Dr. Braithwaite had not used this plan in any case of anteflexion.—*Obstetrical Transactions*, 1878, *p.* 122.

80.—TWO CASES OF INVERSION OF THE UTERUS FOLLOWING DELIVERY.

By Dr. James Braithwaite, Vice-President of the Obstetrical Society of London; Lecturer on Diseases of Women and Children at the Leeds School of Medicine; Surgeon to the Hospital for Women and Children.

Case 1.—Mrs. M., of Harrogate, was confined of her first child on Wednesday, February 20th. There was a slight adhesion which prevented the easy expulsion of the placenta, and which caused a good deal of hemorrhage before its removal. It was not, however, necessary to pass the hand into the vagina in order to do this. If any traction was made upon the cord, it was very slight, and quite insufficient, in the opinion of the medical man who attended the case, to account for the inversion which was discovered to exist on the Friday following. It seems not improbable that the inversion really took place at the time of removal of the placenta, as the patient was at that time quite in a state of collapse, with a feeble or almost imperceptible pulse. Dr. Myrtle saw the case on the Friday, and confirmed the diagnosis made. I was sent for on the following Monday, repeated attempts having been made to replace the uterus, but without success. I found the inversion complete, no rim of cervix remaining. There was no difficulty in examining every part of the tumour by the hand. The depression or cup of the inverted os could be felt through the abdominal walls when the organ was a little pushed up by the hand in the vagina. Making counter-pressure upon this part by one hand on the abdomen, I made the most strenuous exertions to reinvert the uterus, but found it quite impossible to do so, owing to the very firm contraction of the cervix. The rule is to press chiefly at the neck, so as to return first the part which came down last. This cannot always be carried out in practice, for, as in this case, the mass may be so large, that when the fingers reach the cervix on one aspect, the thumb rests upon the fundus, and it is obvious that pressure of the finger-tips in the sulcus between the uterus and vagina surrounding it, without a good grasping power, can be of but little service. The pressure under these circumstances must necessarily be made full upon the fundus. Fearing to do harm by the use of further forcible taxis, we decided to fall back upon continuous elastic pressure. I therefore sent over from Leeds the necessary instruments—namely, a cup of wood, with

slightly-curved stem, the cup being surmounted by a circular india-rubber air-pad, and the stem being set in an india-rubber band, attached before and behind to an abdominal belt. Pressure was kept up by means of this upon the fundus, and attempts at reduction made every day or two, but without any result. I saw the case for the second time on March 6th. For four days previously the pressure had been kept up with great care and very strongly, but no vaginal examination had been made. On introducing the hand, the patient being under chloroform, it appeared at first as if no reduction had been effected, but that the uterus was much smaller. The upper part of the vagina, or what I took to be the vagina, was rather tighter and smoother than the lower and major part of the canal, and this part ended inferiorly at a defined line, but without any ridge or inequality of surface; but it took a moment for the mind to realise that this was really the cervix uteri reinverted, but immensely expanded. The greater part of the uterus was still inverted, but now there was no difficulty in returning it, excepting the last portion, which gave a little trouble. The success of the elastic pressure, the credit of first proposing which is due to the late Dr. Tyler Smith, was in this case complete, and without it I do not believe the uterus could have been reinverted.

Case 2.—I received an urgent message three years ago to see a woman who was said to be dying, and on arrival found she had been delivered half-an-hour before by a midwife who was present. The patient was apparently almost *in articulo mortis.* The midwife had discovered that there was something in the vagina which was unusual; the placenta had come away or been removed; the uterus was found to be completely inverted, no rim of cervix remaining. The hand placed on the abdomen felt what by a superficial or hurried examination might easily be mistaken for a small fundus uteri, but which was distinctly cupped and was the inverted os and cervix. The uterus was grasped and firmly pushed up, and was returned rapidly to its place. The first part which went up was the posterior part of the cervix and the last the anterior part of the cervix, so that a section of the uterus when half returned would be roughly represented by a capital S. In this case the neck had not the small size compared with the fundus observed in the other case, and which was caused by the firm closure or contraction of the part. It is evident that the uterus is not always reinverted in the same manner:—1. The cervical portion as a whole may be returned first and the fundus last. 2. The posterior wall of the cervix may be returned first and the anterior last, or *vice versâ*; this is only likely to happen immediately after delivery, for it supposes a relaxed state of cervix. 3. The fuudus may be

returned first, as in a case in which pressure upon the cervix and fundus by the expanded hand quite failed, but the finger-tips pressed against the fundus caused it to dint in. Continued pressure in the same situation then caused it to spring rapidly back into its natural position. 4. Two or three cases are recorded in which pressure upon one or other cornu of the fundus by the tip of the thumb reinverted this portion, and the rest of the organ rapidly followed its lead, quite springing into its place. Marion Sims relates a case of this kind in which several men of experience in New York had failed, but he candidly adds that his own success was accidental; he was not aware that pressure on this spot would produce the result it did.—*Obstetrical Journal, May* 1878, *p.* 84.

81.—TREATMENT OF POST-PARTUM HEMORRHAGE BY INJECTION OF HOT WATER INTO THE UTERUS.

By Dr. Lombe Atthill, Master of Rotunda Hospital, Dublin.

Post-partum hemorrhage is of such frequent occurrence, and so often assumes an alarming character, that any method of checking it which combines efficiency with ease of application and safety to the patient is certain to be hailed with satisfaction by practitioners. Without doubt the most efficient means at our command for the arrest of flooding after labour is the injection of a styptic, such as the solution of the perchloride of iron, into the uterus. This is a procedure which, after repeated trials, I have no hesitation in recommending, and I shall continue to have recourse to it in suitable cases. Apart, however, from the alleged danger of injecting a powerful styptic into the uterus—a danger which, though well-nigh groundless, suffices to deter many from having recourse to it—there is objection to the practice, that the perchloride may not always be at hand when the emergency arises, and that valuable time may be lost ere it can be obtained.

The introduction of the hand into the uterus—in some cases an efficient method of checking post-partum hemorrhage—is certainly not free from danger, and is moreover by no means reliable in its results. While the routine treatment by cold, whether applied to the surface or injected into the uterus, requires for its success that the patient be possessed of sufficient vital energy to insure reaction. In other words, the application of cold in post-partum hemorrhage is a most efficient remedy in cases where a sudden loss of blood occurs in an otherwise healthy woman, who has not been exhausted by an unduly prolonged labour; but is altogether unreliable, and in many cases positively injurious, where the patient has been debilitated by previous disease, worn out by long protracted suffer-

ing, or exhausted by frequent, though it may be small, losses of blood.

As far as my personal experience goes, those apparently alarming losses of blood which sometimes occur immediately after the birth of the child, or expulsion of the placenta, are not likely to terminate fatally; they can in general be at once arrested by steady pressure over the fundus of the uterus, and by the use of cold, but the hemorrhage to be dreaded is that in which the blood trickles away in a little never-ceasing stream, the uterus relaxing and contracting alternately. This form of hemorrhage, of which I have seen several fatal cases, is most liable to occur in debilitated women, and, in such cases, cold is in general absolutely useless—nay, more, often injurious. I shall never forget a case to which I was called several years ago. The patient had been confined prematurely some hours before; subsequently, hemorrhage set in, not profusely, but it could not be checked by "the usual means," and her attendant becoming alarmed, I was sent for. This patient had been for a long time assiduously treated by the application of cold. Napkins wrung out of cold water had been continuously applied to the vulva, and to the abdomen, cold water injected, &c., but all in vain, a little stream of blood never for a moment ceased to trickle from the vagina, and yet the uterus was firm. When I saw the patient, the whole surface of the body was cold, and the pulse could hardly be felt. My first step was to remove the wet sheet and dripping napkins, and to apply warm jars to the extremities, and I had the satisfaction of speedily seeing the hemorrhage cease, without any further treatment than friction to the fundus of the uterus. From this case I learned that the prolonged use of cold in cases of post-partum hemorrhage may be absolutely injurious, and my practice has been to have recourse to other means if, after a fair trial, cold produce no effect, and not to defer doing so till the vital powers were too depressed.

Another practice I have had recourse to with good results, in certain forms of uterine hemorrhage, not connected with pregnancy, has been the use of Chapman's spinal hot water bags; and, reasoning from the benefit derived from them, in these cases I thought of trying them in post-partum hemorrhage, but from one cause or another I never carried out my intention. My views being thus somewhat unsettled as to the possible value of heat in post-partum hemorrhage, I hailed with satisfaction the suggestion to inject hot water into the uterus in such cases, and decided to put its value to the test of personal experience.

Having decided to try the method, the opportunity was not long wanting. On the morning of the 20th of November a

woman aged thirty-three, was delivered in the Rotunda Hospital of her fourteenth child. Labour had been easy and natural, and pressure, as is the usual practice in this hospital, was maintained over the fundus till the placenta was expelled, which occurred in about fifteen minutes; profuse hemorrhage set in immediately after, and Dr. Smyly, Assistant-Physician to the Hospital, was sent for; he applied pressure to the fundus, cold to the vulva, and injected cold water into the uterus with good effect, but the patient becoming alarmingly weak, he sent for me.

On my arrival in the ward she was almost pulseless, the face pale, and the surface of the body cold. There was little hemorrhage going on, but the uterus relaxed in spite of pressure with the hand on the fundus, and a little stream of blood continuously trickled from the vulva. She was in a state of great danger, and in a condition which would have warranted the use of the perchloride of iron, but instead of having recourse to it, I resolved to inject hot water; this was procured in a moment, and passing the tube of the syringe right up to the fundus of the uterus, I injected water freely at the temperature of 110°, keeping my hand at the same time over the fundus. I was pleased to find that the uterus contracted firmly under it, exactly as it would close had I employed the perchloride of iron. In a very short space of time, probably before I had injected more than a pint of the hot water, the fluid ran nearly clean from the vagina, the pulse improved markedly, and I ceased to inject any more. After a short time the binder was applied; no further bleeding occurred, and the patient made a rapid and good recovery.

On the 30th of November another opportunity offered. A young woman, rather pallid and delicate-looking, was delivered, at 8.45 a.m., of her third child. No hemorrhage occurred at the time, and she appeared to be all right; but at 10.30 o'clock she complained of feeling weak, and was found to be losing blood. Dr. Smyly saw her promptly, and, on removing the binder, found the uterus to be relaxed and flabby, and to reach to above the umbilicus. On using pressure an enormous quantity of clots were expelled, and blood flowed freely from the vagina. Cold was applied, and the uterus contracted firmly. I now saw her; she was very pale, and the pulse could hardly be felt; the uterus was firmly contracted, but remained very large, As the hemorrhage seemed to be checked, I did not at first think that more need be done, but while my hand was still on the fundus I perceived it to relax, and blood flowed again. I therefore decided on injecting hot water before matters became worse, for it was evident that she could bear very little further loss.

On introducing my fingers up to the os, with the view of guiding the tube of the syringe into it, I ascertained that the great size of the uterus was partly due to the presence of a fibrous tumour in its anterior wall. This fact did not deter me from proceeding with the injection, but rather decided me to do so. As in the previous case, the water no sooner reached the fundus than the uterus contracted firmly, and the oozing of blood ceased; but the most remarkable feature was the immediate effect on the pulse—it at once improved markedly, becoming fuller and stronger. The uterus did not relax again, and no further loss occurred. This patient recovered strength very slowly, but was able to get up in a week. On questioning her subsequently as to her feelings at the time, she stated that at the moment of the injection she experienced the greatest comfort and obtained immediate relief from intense pain from which she had up to that moment suffered.

I am unable to say to whom we are indebted for the introduction of the use of hot water injections for the control of uterine hemorrhage. I was induced to try it in consequence of a letter written by Dr. Whitwell, of San Francisco, to Dr. Foley, of Boston, who is at present studying in this hospital. Dr. Whitwell's statement is to the following effect:—When house surgeon at the New York State Women's Hospital, he saw the uterus contract firmly and instantly upon being washed out with hot water after an operation by Dr. Marion Sims, upon a sarcomatous growth of the fundus uteri. This was in 1874. The result led him to try the same treatment in post-partum hemorrhage, where he met with perfect success. He afterwards succeeded in having the treatment tried in the Lying-in Hospital at Prague, where he studied for some time. At first he was ridiculed, but as the method was successful in every respect it was adopted as a regular routine treatment. The water should be at a temperature of 110°. I had previously seen the treatment alluded to in some of the periodicals, but this letter decided me to test it personally.

The first notice which I have been able to trace relative to the use of hot water injections in post-partum hemorrhage occurs in the American Journal of Obstetrics for April, 1876, in which, in an abstract of a paper by Dr. Carl von Rokitansky, jun., "On the Treatment Employed in Vienna for Uterine Hemorrhage," the following brief passage occurs:—"Dr. Windelband has recently recommended injections of hot water in menorrhagia and post-partum hemorrhage," and nowhere else have I been able to meet with more explicit directions on the subject.

My experience is as yet but scanty, still I have twice used hot water injections in cases of sufficient gravity to call for the most prompt and active treatment, and with most satisfactory results.

Both were examples of that class of post-partum hemorrhage which are liable to end fatally, not indeed immediately, but in the course of a little time, if the thin stream of blood be not stayed. Since I became Master of the Rotunda Hospital two such cases ended fatally.

The cases I now record undoubtedly establish this much—that the injection of hot water powerfully stimulates the uterus to contract, and thus rapidly checks the hemorrhage; but that it does more is, I think, as clearly established; it evidently acts as a general stimulant. The effect on the pulse was most marked; indeed the pulse was affected more rapidly than by the hypodermic injection of ether, and it did not flag again. The faces of the patients, too, lost the deadly hue they previously had worn; and last, not least, they expressed themselves as having experienced the greatest relief, and obtained great comfort.

I anticipate very good results from the introduction of this simple treatment into obstetric practice. There are many men who fear to use the perchloride of iron as an intra-uterine application, but who, when prejudice has been overcome, will not shrink from the use of hot water; and, I believe, not a few lives may thus be saved. To recapitulate, in hot water we have at once a safe and efficient remedy, one comforting and agreeable to the patient, and an agent which is always at hand. Whether it is as reliable as the perchloride of iron remains yet to be proved.

It should be remembered that the advantage to be derived from the intra-uterine injection of hot water is not confined to cases of post-partum hemorrhage. It was first used to check hemorrhage occurring in cases of chronic disease of the uterus, and after operations. Its range of usefulness is therefore great. At the present time I am engaged in observing its action in chronic cases, and hope in time to be able to bring forward the results.

In conclusion, I would urge practitioners to try for themselves the use of hot water injections. I can assure them that they need not fear bad results; nor, though 110° is the temperature named, is accuracy in this respect needed. Water in which the hand can be kept without discomfort may, with safety, be employed; but, it must be remembered, that if the temperature be allowed to fall much under 110°, disappointment will certainly follow; equally will the injection be well-nigh useless if the tube of the syringe be not passed right up to the fundus of the uterus, or at least fairly to within its cavity.—*Obstetrical Journal, May* 1878, *p*. 126.

82.—ON A NEW PREPARATION FOR ALLAYING IRRITATION OF THE ACTIVELY SECRETING MAMMARY GLANDS.

By Dr. Hugh Miller, Physician-Accoucheur, Glasgow Lying-In Hospital.

The treatment of the breasts by an application of the active principle of either the belladonna leaf or root is not a new proposal. As far as I have been able to ascertain, it was introduced to the notice of the profession in the Dublin Medical Journal of 1834. Since then, one or two short notices of its value in affection of the mammæ have been contributed to the journals. In 1860, Dr. Marley gave a statement of his views on the efficacy of the drug for this purpose to the Obstetrical Society of London. While belladonna was generally recognised by these authors as a reliable local sedative in the treatment of painful affections of these glands, it remained for Dr. Fordyce Barker, in his excellent work on Puerperal Disease to point out more clearly its value in acute affections. In his experience, "belladonna not only relieves the pain resulting from the tension of the tissues, but from its power of relaxing muscular fibre, it seems to allow a more free exit of milk by dilating the lactiferous tubes; and within a few years past, it has been believed to possess the property of arresting the lacteal secretion. But of this I am certain, that it is a most valuable application to the breast in glandular mastitis, and I have used it for this purpose for more than twenty years."

The cases in which the preparation has been applied, are those in which acute congestion occurs in the mammary glands when beginning actively to secrete. The sudden and copious flow of milk is accompanied by the determination of blood to these organs, rendering them peculiarly liable to inflammatory affections; and this condition seems to be more readily induced, when, either through sore nipples or other causes, the female breasts are prevented from efficiently performing their function as excreting glands. With most mothers, on the second or third day at latest after the completion of labour, the breasts are observed to become firmer and sensitive to the touch. It is a condition which may be temporary, or one which a saline draught may readily relieve. Its continuance depends on the activity of the lacteal secretion. Should the flow of milk continue,—and in the majority of cases I have met with it has done so,—the secretion must either be withdrawn by the infant, or in those cases where this means is not available, the plan hitherto has been to continue the saline, and to allow of a certain amount of engorgement taking place in the hope that the secretion will gradually cease in the absence of the stimulus of suction. In my experience, in the majority of such cases, the secretion goes on and the breasts continue to increase

in size; when still left alone they become hard, more painful to the touch, and at length the distension is so excessive as to excite inflammation. In the event of suppuration occurring, one gland usually becomes affected in the first instance, and the disposition to congestion generally spreads rapidly, involving the whole of the glands, and often the connective tissue surrounding them.

This condition of the mammary glands was a frequent source of anxiety to me until the perusal of Dr. Barker's Lectures—suggesting belladonna—led me to adopt the plan of treatment which I now propose to lay before you. For some time I had been dissatisfied with my management of the breasts where an active treatment of them had to be employed. I had used the various liniments and ointments, and I was satisfied that frequently only an imperfect trial was given to the remedy, since complaints were made that repeated frictions could not be persevered in owing to their increasing instead of relieving the pain; and in those cases where rubbing in the remedy was an essential to the treatment, I thought the objection, when urged, was a reasonable one. With a view to avoid friction, and to secure the full therapeutic effect of the belladonna, I had an alcoholic extract prepared of double the strength of the Emplas. belladonnæ, but kept fluid by collodion. Camphor was combined with it for the purpose of aiding to arrest the natural mammary secretion. This preparation, now shown, is painted on the breasts much in the same way that you would use blistering fluid. No rubbing in is necessary. The fluid dries quickly, is much more cleanly for the patient, has a less offensive odour than the ointment, and, in my experience, it is more reliable in its action.

This liquid preparation is painted over the affected parts of the breast night and morning, until the acute symptoms give in. Indeed, it can only be of service as a good local sedative when the free and frequent application of it to the affected part has been persevered in until decided results are secured. During the past, I have used this preparation with very satisfactory results. Whether the inflammatory irritation accompanying the onset of the lacteal secretion had for its exciting cause exposure to cold, inflamed nipples, or obstruction in the lacteal ducts, the preparation has always seemed to be of value. I have also used the preparation beneficially by applying it to both breasts every day when the mother did not intend to suckle her child: and from the frequent opportunities I have had of observing the result, I am satisfied that it may be safely relied upon for restraining the secretion of milk, and acting on the walls of the arterioles so as to prevent engorgement. It has the advantage over the

old plan of evaporating lotions, in that it is more cleanly, and is more comfortable to the patient. When the remedy is employed to prevent the secretion of milk forming at all, I have found it best to begin applying the liquid from immediately after the birth of the child. I anticipate the lacteal secretion, and endeavour to prevent its formation. The Emplastrum belladonnæ liquidum has hitherto given very satisfactory results in these cases. Whether this result would have been so satisfactorily accomplished had I waited until the breasts began to secrete milk, I am unable to say. When endeavouring to allay any irritation of the glands by the external application of this fluid, I push the remedy until a decided local effect be secured. In such cases I paint the breasts daily or oftener. I also insist upon the patient giving the whole organ rest by remaining in the recumbent position, and having the breast properly bandaged. The milk, when present, should be periodically drawn off until the organ returns to its healthy state. I may add that, should it become necessary to relieve arterial tension, a small dose of aconite frequently repeated will be necessary; and when sympathetic fever accompanies the disorder, a saline should be given.—*Edinburgh Medical Journal, Dec.* 1877, *p.* 491.

ADDENDA.

83.—QUINETUM AND ITS THERAPEUTICAL VALUE.

By Dr. H. J. VINKHUYSEN, Physician to the Household of H.M. the King of the Netherlands.

Though the history of quinetum would present to the impartial reader an excellent illustration of the difficulties which have to be overcome in order to gain general acceptance for a scientific truth, I do not propose to enter upon it here, but will confine myself to relating in a few words the conclusions to which I have been led by a practical inquiry into the actions and uses of the remedy.

The name quinetum was given by my illustrious compatriot, Dr. de Vry, to the collective alkaloids obtained from Peruvian bark by a very simple process. By repeatedly inviting medical men to test the utility of this medicine, by supplying them with pure specimens of it, and searching for the best forms of prescribing it, he induced several physicians at the Hague, and throughout Holland, to use it extensively in several cases where the salts of quinine had been habitually employed.

I have prescribed it in a hundred different cases, and from this number, I think I am fairly entitled to draw some definite conclusions. I shall narrate several of these cases at length, as they afford instructive examples of the action of this remedy.

1. Lady J. suffered from tertian ague, with great disturbance of the nervous and circulatory systems. This patient could not tolerate quinine on account of the unpleasant symptoms which were invariably produced by it, even when the dose was very small. These symptoms consisted in great præcordial anxiety and asthma. When hydrochlorate of quinetum was given it did not produce the slightest trace of these symptoms, even when administered in doses twice as large as those of sulphate of quinine. The good effect, however, was strongly marked, and complete disappearance of the febrile symptoms in a very short time could be observed.

2. Mr. van Lonsberge, our present governor-general in the colonies, was a martyr to facial neuralgia, for which he took sulphate of quinine. This invariably caused him great suffering by bringing on noises in the ears, which were so loud that it became quite impossible for him to work, or even think, on any day that he took quinine to relieve the intense neuralgic pain.

In his case quinetum relieved the pain even more quickly than quinine, without bringing on the hallucinations of hearing which he so much dreaded.

3. Mrs. S. N., during the early months of pregnancy, suffered from œdema of the feet, retching and disturbances of vision, which were the precursors of a violent attack of eclampsia. She was successfully treated by depletion, and the eclamptic symptoms disappeared, but an abnormal frequency of the pulse still remained. During the whole day the number of pulsations was never less than 130 per minute. This symptom appeared to indicate quinine, but we were afraid to prescribe the alkaloid on account of the symptoms of congestion, which it is apt to produce. As the action of quinetum resembles that of quinine, I was induced to give it instead of the latter in doses of four grains a day. The result justified my expectations, for in three days the pulse sank to seventy-two.

There is, perhaps, no country where malarious intermittents are so common as in Holland. The effects of malaria do not always present themselves as intermittent fever with regular paroxysms, or as pernicious fever with dangerous symptoms. We often meet with affections of the most diverse kinds, which are connected only by having the malarious poison as a common cause.

The proteus-like forms of malarial poisoning are known under the name of "larval" or "masked malaria." In these affections quinine has not the same powerful, therapeutical action as in cases which are acute and obviously due to the action of the poison. In the greater number of them we are obliged to have recourse to quinoidine in combination with quinine. It was therefore natural to try quinetum in these cases, and experience showed that it effected a more radical cure than quinine in chronic malarial intoxication.

4. Mr. R. complained of pains in the legs, having the character of sciatica, which were brought on by walking after sunset, and which recurred at the same time of day. By taking quinine with aconite the pains were relieved for three or four days, but after that time they returned. On giving him quinetum for three weeks they vanished altogether.

5. It is very interesting to compare the action of quinine and that of quinetum on the temperature of the human body. For this purpose I administered quinetum in a case of petechial typhus on the tenth day, when the temperature of the patient was 41°C. Three grains were given in divided doses, one grain being given every half-hour. After two hours the temperature had fallen to 38·2°C., without any of the disagreeable symptoms that we usually observe when quinine is employed.

6. It is a common custom amongst men who have passed

several years in tropical regions to take several grains of quinine when they suffer from a feeling of general weakness. After the quinine they feel quite well again. This tonic effect of small doses of quinine is observed still more markedly when quinetum is employed.

7. The son of Mr. T. suffered from quotidian ague after very intense bleeding from the bronchial vessels. Quinine caused intense vomiting, which was very dangerous under the circumstances, and as the vomiting could not be checked, I gave up the quinine and used quinetum instead. The result was highly satisfactory, for the quinetum did not cause the least vomiting, and had the desired effect of checking the ague.

From the observations I have made, and from which I have selected the above, I believe that we may affirm with perfect safety the following conclusions:—

1. The only malarious disease in which quinetum cannot be employed in place of quinine is pernicious fever. Quinetum requires more time to act than quinine, and as rapidity of action is absolutely necessary in this disease, quinetum cannot be used in it as a substitute for quinine.

2. In all forms of pure malarial intermittent fever, quinetum has the same apyretic effect as quinine, but is less powerful and acts more slowly. It must therefore be given in large doses and at longer intervals before the ague fit, than quinine.

3. Quinetum does not produce the unpleasant and even dangerous symptoms of quinine when given during the fit, and may be taken during the fit without causing any unpleasant feeling.

4. Quinetum never causes noises in the ear.

5. Persons who are liable to suffer from the toxic effects of quinine, and who therefore cannot take it without the greatest discomfort, can take quinetum without this unpleasant effect, and yet obtain a similar therapeutical result.

6. The influence of quinetum in chronic cases is greater than that of quinine.

7. The tonic action of quinetum is similar and perhaps even greater than that of quinine.

8. The action of quinetum in cases of masked or larval malaria, and especially in rheumatic affections due to malarious influences, is incomparably greater than that of quinine.—*Practitioner, Feb.* 1878, *p.* 81.

84.—ON THE ACTION AND USE OF HYOSCYAMINE.

By H. Clifford Gill, Esq., Resident Medical Superintendent, Lunatic Hospital, York.

I purpose to discuss some of the actions and uses of the alkaloid hyoscyamine, or rather, in the form in which I use it,

an extract containing the alkaloid in an amorphous form.* It is of a dark, olive-green colour, and about the consistence of treacle. It is readily soluble in alcohol and ether, but more freely, I think, in a mixture of these two. Of the mode of preparation of this valuable drug I shall say nothing; but any one wishing for further information on this head will find a full description in the Journal of the Pharmaceutical Society for August 23, 1873. Of this extractive I make a solution of a given strength for dispensing purposes. There is also a crystalline alkaloid hyoscyamine and its sulphate corresponding to atropine and its sulphate; but, therapeutically, I am led to believe there is no great advantage to be gained, and the cost is at present almost prohibitive—3s. 6d. per grain. While on this branch of the subject I should state that, for reasons to be afterwards described, it is very desirable to give the alkaloid in a very freely diluted state, or otherwise unpleasant symptoms referable to the stomach are apt to arise. This solution contains exactly half a grain of hyoscyamine to the ℥j. The formula which from experience I have found most convenient is: hyoscyamine gr. x., sp. vin. rect. ℥j., æth. sulph. ʒ iv. The two latter are mixed, and this mixture will freely dissolve the hyoscyamine; then make up the quantity to twenty ounces with water. The solution is quite clear and of a bright brown colour, and of a disagreeable pungent odour and taste. The reason I have adopted the half grain to ℥j is to simplify the administration of the medicine in doses which are multiples of one-eighth of a grain, which of course affords facility for dispensing. Two precautions should be taken; first, not to keep the solution too long, as it changes its properties and becomes nearly inert; and second, to avoid prolonged exposure to light by keeping it in a dark place. With regard to its administration, from an experience of my own of sixty-five recorded instances of administration, I can recommend a dose varying from one-eighth to three-eighths of a grain, and in exceptional cases three-quarters, or even a grain may be given with perfect safety. I never give three-eighths of a grain in less than two ounces of water, as vomiting is sure to follow, and perhaps, even, as occurred at Wakefield, hæmatemesis, the drug when concentrated appearing to have a powerful local action on the mucous membrane of the stomach. So much then for the drug and the mode of its administration. I will now proceed to discuss its use. If a moderate dose of hyoscyamine be given to a healthy man he will exhibit many of the phenomena of an attack of mania, plus certain well-marked physical conditions; he becomes loquacious, incoherent, rambling, and has certain

* Merck's amorphous hyoscyamine, supplied by Messrs. Harvey and Reynolds, of Leeds.

well-marked hallucinations of vision and audition, great weakness, especially of the lower limbs, and considerable loss of co-ordination, similar to that seen in a drunken man, intermittent drowsiness, hypermetropia, dryness of lips and throat, and, not uncommonly, vomiting.

Now it was once thought that if in a person already the subject of disorganised brain action another process could be induced, a reaction might take place, and a changed condition for the better be the result. Be this as it may, as a fact, great benefit and amelioration does take place in many classes of cases, and this too when all the more common forms of narcotics, such as the various preparations of morphia, chloral, cannabis indica, conium, &c., not forgetting the universal neural panacea, bromide of potassium, have been tried and have proved failures.

Many doctors in general practice must frequently be called to cases of acute mania in their early stages, when it is that extreme violence in a private house is so fraught with danger both to the friends as well as to the patient: in such cases I think great benefit would be derived by the administration of a full dose of hyoscyamine, and even if, as is most likely to be the case, the attack is not cut short, yet the patient is calmed and sleeps quietly till other steps can be taken for his after treatment. So again many patients suffering from dementia, who are for the most part harmless and who live with their friends, are now and then liable to attacks of acute brain irritation and become very troublesome, noisy, violent, and dirty. In such as these I think much benefit will be found from this drug given at first in a full dose, three-eighths or three-quarters of a grain, and continued afterwards in one-sixteenth to one-eighth of a grain dose. As a suggestion it might be quite worth trying in delirium tremens.

In conclusion I would add that there are certain precautions to be borne in mind having regard to the fact that the drug under consideration is one of the most potent we possess.

Be careful how you give it to patients with degenerate arteries or chronic cardiac disease, or in very old people. Dilute the drug freely, for the reasons I have previously mentioned (though I must confess that in most of my cases vomiting followed, though very freely diluted). Use the preparation fresh, and keep it when not in use in a dark place.

Dr. Lawson also advises that it be not administered in cases of furious mania, where feeding by the pump is likely to be required on account of the dryness produced in throat bronches, and hence the subsequent difficulty in passing the tube. The solution of the crystals can be used hypodermically, but at present I have not been able to give the hypodermic method a trial.—*Practitioner*, *Feb.* 1878, *p.* 85.

85.—THERAPEUTIC USES OF EUCALYPTUS GLOBULUS.

By Benjamin Bell, Esq., F.R.C.S.E.

[The Eucalyptus is the Blue Gum tree of Tasmania, discovered in 1792. Nat. Order Myrtaceæ.]

As Gubler has shown, the anti-catarrhal virtues of *eucalyptus* are most remarkable. With increasing experience of its power, I more and more employ it in bronchial, vesical, and uterine catarrh, in gonorrhea and in gleet.

These representations of Sir John Rose Cormack, and the circumstance which he also mentions, that a preparation of the essential oil in capsules is a favourite prescription with many leading physicians in Paris, led me to make trial of the remedy in a variety of cases during the past year. The only preparation which I have used has been the tincture prepared by several of our most eminent druggists in Edinburgh, and I have seldom prescribed more than a teaspoonful, mixed with a wineglassful of water, twice a day.

In several cases of bronchitis with profuse expectoration, I have witnessed remarkable benefit after a very brief use of the remedy, evinced by a rapid diminution of the discharge, and also by a corresponding improvement in the general condition of the patient. But my object in writing this note is to recommend the internal use of *eucalyptus* in a class of cases to which, as far as I know, it has not hitherto been considered applicable.

It occurred to me, that a plant which has obtained a great reputation in Tasmania, Australia, New Zealand, the south of France, and in Italy, as a defence against malaria, and which evidently possesses valuable properties as a disinfectant, deodorant, and astringent, might prove useful in certain forms of disease in the stomach and bowels. I have not been disappointed in this expectation, and would therefore encourage my professional brethren to give it a fair trial in such cases.

A gentleman of seventy-five had suffered from formidable disease of the stomach for eight or ten years, and on several occasions had seemed very near his end, with every symptom of malignant ulceration. Great quantities of blood had been vomited from time to time, and at short intervals, seldom exceeding a fortnight, the stomach after becoming painfully distended with a sour *barmy* fluid, was relieved by repeated vomiting, while life itself seemed possible only with extreme lightness of diet and most vigorous self-denial. A strong, active man had become a confirmed invalid, and seemed both to himself and others beyond the reach of remedies. He has taken the tincture of *eucalyptus* twice daily for many months, and during all that time has scarcely had even a threatening of those painful and exhausting attacks which had latterly occurred almost every week.

Another old gentleman, a retired medical man of eminence, who for some years has laboured under symptoms which indicate disease of the stomach and possibly the colon, is so sensible of benefit from the use of the medicine, that he can seldom abandon it for even a few days without being reminded of its importance and eagerly resuming it.

Another case in which ulceration, or some other organic disease of the stomach seemed the only reasonable diagnosis, the patient, a widow with a family dependent upon her, made an unexpected recovery from extreme attenuation and weakness under similar treatment.

I have tried it repeatedly in a class of cases which are usually regarded as ulcers of the stomach threatening perforation. They are mainly young women, servants, sempstresses, and others, who partake largely of tea which has been boiled or allowed to infuse too long on the hob; but I cannot say that their attacks of pain after food have been cut short in the same indisputable manner as in the cases already referred to, because such persons usually get well under ordinary treatment, and by avoiding what may be called the poisonous cause of their malady. Still, I have met with individnals who by taking the *eucalyptus* have become, in their own opinion, exempted from the recurrence of attacks, which past experience had led them to anticipate.

My object in this note will be attained, if those who have opportunities will give the remedy a trial; for it is only by an unprejudiced and cautious accumulation of experience that the real value of a therapeutic agent can be estimated. Of course, no one will think of using the medicine as *a specific* in any case where it may seem to be indicated. All the details as to diet and general regimen, which would be deemed necessary without it, must be carefully attended to.

In conclusion, I may say, that it seemed to me of manifest use lately in a case of diphtheria, commencing in the gullet and ascending to the fauces; and my belief is, that it might be prescribed with advantage in some cases of typhoid fever.—*Edinburgh Medical Journal, Feb.* 1878, *p.* 681

86.—NITRE-PAPER FUMIGATION AS AN EXPECTORANT.

By Dr. Dyce Duckworth, St. Bartholomew's Hospital.

I believe the practice of nitre-paper fumigation originated in France, and is much employed in the bronchitis of the aged in that country. Trousseau commended the use of this and other fumigations in several pulmonary affections, notably that of arsenical preparations in cases of neurotic asthma.

I first learned the value of it from the teaching and practice of that eminent therapeutist, Dr. Nevins, of Liverpool.

The class of cases in which resort may be had to this means is chiefly that of bronchitis in aged and enfeebled persons.

The last few weeks have presented most practitioners with a large field for the use of fumigation-therapeutics, for polar winds have cruelly devastated the greater part of Europe, and laid prostrate whole hosts of people with every degree of bronchial trouble.

We are only too familiar with cases of senile bronchitis in which prolonged fits of violent and exhausting cough are ineffectual to the clearance of the bronchial tubes and trachea. With all our best efforts in the direction of stimulating expectorants and pneumo-musculo excitants, we not seldom fail to afford relief to such sufferers.

The irritation induced in these cases is sometimes sufficient to induce very definite asthmatic paroxysms, lasting either a few minutes or for two or three hours.

We are then presented with an indication for the employment of remedies such as stramonium, belladonna, or even lobelia. We may give these at night, for it is found that this untoward complication is most apt to occur during the night, the patient being awoke from sleep by an attack of ineffectual, although prolonged, cough. Given either by way of pill in anticipation of such an attack, or by the method of inhalation according to the Pharmacopœia formulæ for the various *Vapores*, at the time of the paroxysm, we may endeavour to give relief.

In any such case it will be well to try the effect of nitre-paper fumigation. It is found that the inhalation of the products of its combustion excites usually effectual cough, acts as a very efficient expectorant, and allays spasm.

Some years ago I recommended to Messrs. J. Bell and Co. that they should paint over this nitre-paper several coatings of Friar's Balsam, and I have found this to be a very useful addition. The paper ought to be the coarsest brown paper procurable, the old cordage in it proving of value in giving off tarry empyreumatic fumes, and these together with the oxygen gas evolved from the potassic nitrate, and the benzoic acid from the compound tincture of benzoin, form a suitable stimulant and balsamic smoke for inhalation.

The bed-curtains should be drawn around the patient while the paper is burnt, so that a fully charged atmosphere may be inhaled, and a piece of paper not less than four or five inches square should be used each time. The process may be repeated several times a day.

The great advantage of this medication is its simplicity; it s also within the reach of all classes of patients.

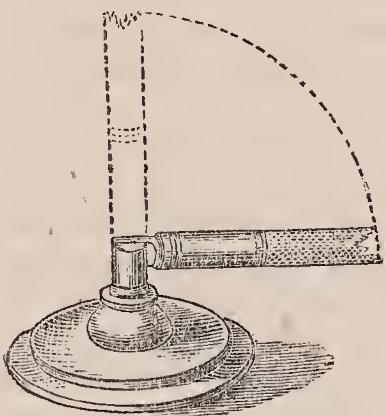
I would not close this brief communication without remarking that the distinct value of warm cordial drinks is somewhat

lost sight of as a powerful expectorant means. A cup of hot tea or coffee with a little brandy or essence of ginger, or a draught of hot water to which some essence of peppermint or spearmint is added, is eminently effectual as an expectorant by stimulating the gastric branches of the vagus in the first instance.

Some such drink should be given during the night, or early in the morning, to aged and enfeebled sufferers from bronchitis.—*Practitioner, May*, 1878, *p*. 321.

87.—A NEW ALUMINIUM TELESCOPIC STETHOSCOPE.

Messrs. Salt and Son, Birmingham, have constructed a light, elegant, and portable stethoscope, of good acoustic properties. The ear piece is of ivory, and is made either to unscrew or to move on a hinge at its junction with the stem, as shewn in the annexed engraving. In the latter instrument, the ear piece, when in use, can be kept firmly in position by means of a sliding tube. The stethoscope itself can be drawn out like a telescope to a length of six inches, and prevented from collapsing when in use by means of a bayonet lock.—*Lancet, March* 23, 1878, *p*. 426.



88.—ON THE USE AND ADMINISTRATION OF FAT.

By Dr. T. Lauder Brunton, F.R.S., &c.

In a paper read by Dr. Brunton before the Medical Society of London, he began by referring to the feats of Johnson and Captain Webb in swimming across the Channel, remarking that the greatest difficulty they had to contend with was loss of heat, so that Webb's success may be attributed to a thick coating of subcutaneous fat, aided by the porpoise oil with which his skin was lubricated. In some animals in tropical climates the fat is collected in large masses at certain parts, as in the zebra, the yak, the buffalo, and the camel. In the camel the hump is firm and solid when the animal is well fed, but when insufficiently supplied with food it becomes loose and flabby. Whenever the fat assimilated is insufficient for the wants of the economy, the subcutaneous fat becomes absorbed in order to supply the waste of more important tissues. Fatty, starchy, saccharine, and albuminous articles are all capable of being converted into fat. The fats themselves are partially emulsified, as well as partially saponified by the pancreas, and, passing through the lacteals,

mesenteric glands, and thoracic duct into the general blood-current, increase the amount of fat in the blood. Other articles of food will increase the proportion of fat, when not taken in excess. The late Dr. Hughes Bennett used to say: "The main causes of tuberculosis were the dearness of butter and the abundance of pastry-cooks, the poor not getting sufficient fat, and the upper classes disordering their digestion by puff-paste." The author thought that the instinct of those children who refused to eat fat was perfectly right, and said that if fat be swallowed under compulsion it would generally disagree; and in such cases the proper thing to do is to give fat in another form. The more finely the fat is subdivided the more is it enjoyed, and the more readily is it digested. A piece of solid butter swallowed alone would melt in the stomach, float about without undergoing digestion, and probably begin to decompose and yield acrid fatty bodies, which would irritate the stomach and cause vomiting. The best time to give cod-liver oil is an hour or so after, instead of immediately after, a meal, as it will then have a shorter time to stay in the stomach, and will pass out quickly into the duodenum. The oil is better given in the form of an emulsion with gum acacia, than with solution of potash or carbonate of potash, because the gum is little, if at all, affected by the gastric juice, whereas the p otash willbe neutralised, and its emulsifying properties destroyed, so that the particles of oil can again run together. Many opinions have been advanced regarding the ready digestibility of cod-liver oil. Some have sought for its cause in the fact that this oil contains propyline in combination with fatty acids, instead of glycerine, like most other fats. Others, again, have attributed it to the minute quantities of iodine, and others to the biliary matters found in the oil; the last seeming to be, by far, the most reasonable supposition. It appeared to the author that in bronchitis, both acute and chronic, a little cod-liver oil is much more serviceable than cough mixtures, as well as in phthisis, in which disease it is so generally employed. After a trial of various things, the author has found that fat bacon is one of the most satisfactory foods upon which to perform hard mental work. The nervous system, containing a great proportion of fat, must necessarily suffer if fat be deficient in the food. He believed that the glycosuria of gouty subjects was due to the same cause as the excessive production of urates—viz., deficient oxidation; for the sugar and the excess of uric acid may alternate in the urine, or both may be wanting, and then fat is deposited. Seegen has noticed that diabetes is often preceded or accompanied by an immense accumulation of fat; and the gouty glycosuria of middle age often occurs in very stout per-

sons. Whenever the supply of oxygen to the tissues is diminished, either by depriving the blood of its red corpuscles or by lessening its flow to a part, deposition of fat or fatty degeneration is likely to ensue. This may be observed in women after excessive flooding or in the muscles of a paralysed limb, or in the heart when it has grown too large to be nourished completely by the coronary arteries.—*Lancet, Dec.* 15, 1877, *p.* 885.

89.—A SUCCESSFUL MODE OF TREATING NÆVUS.

By Andrew Bontflower, Esq., Hon. Surg. to Salford Hospital.

The surgical treatment usually adopted, and recommended in that form of nævus which involves the entire tissue from skin to mucous membrane, is far from satisfactory. Caustic irritants, various kinds of seton and ligature, removal by the knife, galvano and thermo cautery, injection with carbolic acid, all have their advocates. The objections to the caustic applications, such as iodine and chloride of zinc. if they cure the nævus, consist in the fact that ugly cicatrices in the skin ensue—setons in large nævi very rarely cure the disease; removal by cutting away the diseased portion, is apt to be attended by serious hemorrhage, and in a child by convulsions; the galvano cautery has not been attended by the brilliant results which were anticipated, and the attempts to set up inflammation in the part by injections of carbolic acid and perchloride of iron have more than once terminated fatally.

Two interesting cases of venous nævi, presenting in some degree the same characteristics have recently come under my care, in each case the disease extended from skin to mucous membrane—the one being an adult, with the nævus in the upper eyelid, the other an infant with the disease occurring in the upper lip. Both were treated in precisely the same manner, a mode of treatment which I do not remember to have seen recommended, and therefore perhaps worthy of record.

The first case was that of a child, two years old. Previously to my seeing him, he had been under the care of two medical men in Rochdale, who, I was informed, used local applications and setons. The entire upper lip was involved, and hung in an ugly swollen condition over the lower lip. The nævus was not congenital, had recently developed, and as the parents stated, was increasing rapidly. Assisted by Dr. Crompton, I passed three acupressure pins horizontally, at equal distances, and parallel to each other. After puncturing the skin with the point of the pin, I passed it on in a rotatory kind of way, something like darning a stocking, backwards and forwards, taking care to keep the point away from the skin and mucous

membrane, until it emerged at the opposite side of the lip. Endeavouring in this way to secure compression of all the veins, and at the same time to include them all in the grasp of the pin. This I believe would have been sufficient, but in order to make quite certain I passed a ligature of worsted, in the figure of eight fashion, over the pin, but not sufficiently tight to impede the circulation in the skin. The worsted was allowed to remain three or four days; but the pins were not removed for a fortnight, the parts gradually shrunk, the veins became obliterated, the darkened purplish skin resumed its healthy hue without any cicatricial marks—and in about eight weeks the lip had so far resumed its natural size and shape that it would have been difficult to trace any sign of the nævus. No convulsions, local inflammation or symptoms of constitutional irritation at any time supervened.

The other case occurred in a young woman whose appearance "before and after" is shown in the accompanying illustration, and was congenital, but she always trembled at the thought of an operation. She was admitted into the Salford Hospital and I treated her case in exactly the same way as the previous one. The only difference in the progress of the case being that slight erysipelatous inflammation over the forehead and temple showed itself on the fourth day, but this quickly subsided on the removal of the worsted.

This method of procedure seems to me so highly satisfactory in its results, so entirely free from any kind of danger and at the same time so eminently simple, that it will probably meet with general approval.—*Liverpool and Manchester Medical and Surgical Reports*, 1878, *p*. 117.

90.—IODOFORM AS A LOCAL APPLICATION.

By Dr. Wyndham Cottle, M.A., Senior Assistant Surgeon to the Hospital for Skin Diseases, Blackfriars.

Iodoform has been in extensive use for some time by many surgeons, and, during the last two or three years, I have experimented with and prescribed it largely and with the most encouraging results. First discovered about the year 1824, by Serullas, its properties have long been known to chemists. It is readily obtained by adding an alcoholic solution of potash to tincture of iodine, and crystallises as a yellow lustrous coarse-grained powder of a peculiar pungent penetrating odour. It stands in the same relation to its analogues chloroform and bromoform as hydriodic acid does to hydrochloric and hydrobromic. It may be regarded as chloroform ($CHCl_3$), in which the three atoms of chlorine are replaced by three of iodine (CHI_3). It also forms substitution compounds with chlorine

and bromine. It is sparingly soluble in water and glycerine, less sparingly so in alcohol and warm oil, but readily soluble in ether, and to a still greater degree in chloroform.

Two years ago, I made solutions of iodoform in alcohol and ether; but, as the liquid rapidly became of a dark iodine tint, I feared that some substitution-product or decomposition might take place in the liquid, and generally employed the solution in warm oil. Chloroform is, I believe, its most effective solvent. Iodoform can readily, by trituration, be made into an ointment with either lard or vaseline, and its odour, in some measure, disguised by the addition of essential oils, as the essential oil of almonds. As a powder, it can be employed alone or diluted with fullers' earth, magnesia, or tannin; the last mentioned body having the peculiar property of, in some measure, removing its powerful and disagreeable odour.

In the form of suppositories, iodoform has been employed as a local anodyne, but with no marked benefit, as I understand, though I have no experience of it in this respect. As a powder, it has been extensively applied to cancers and venereal sores; and I have to thank my friend, Mr. George Perry, for calling my attention to its use in these cases, when I was at once struck by the very remarkable results produced. Its action can, perhaps, be best shown by stating its effect in the several affections in which I have applied it.

Venereal Sores.—Iodoform seems to act equally well in these cases whether they are ordinary venereal sores or genuine hard or soft chancres, and whether situated beneath the prepuce or on other parts. Its action seems to be that of a topical irritant in some measure, and it may set up too much local action, if applied to an inflamed sore or wound, as Mr. Berkeley Hill points out in his paper on the subject in the British Medical Journal of January 26th, 1878. It should not, therefore, be applied to a sore when acutely inflamed.

From records of cases, I find that twenty cases of venereal sores classed "primary syphilis," which occurred in practice in their chance sequence and without any effort at selection, were treated by me by the ordinary local methods, with or without internal remedies. These were, on an average, rather more than twice as long under treatment before the sores were completely healed, as the same number of other cases, taken in a similar way and under precisely similar conditions, in which the only remedial measure was the topical application of iodoform. These results are the more encouraging when I add that, in patients so treated, there is diminished risk of buboes and lessened constitutional depression from the more rapid progress of the cases. It seemed to me also that the sequence of secondary syphilis was less frequent. Iodoform acts particularly well in cases where there is a disposition to slough.

Buboes, Syphilitic Ulceration, &c.—In practice, buboes that are most tedious and indolent are of frequent occurrence. They often have deep and extensive sinuses and fissures that show little or no inclination to heal, and sorely tax the patience both of the surgeon and of the patient. I have found that these cases almost invariably rapidly granulate, contract, and cicatrise by the application of iodoform; and the same obtains in the late forms of syphilitic ulceration. A man, about twenty-eight years of age, with serpiginous ulceration of syphilitic origin, which, first breaking out in the groin, had extended over the lower part of the abdomen and upper part of the thigh, and was for over a year under treatment, with every likely remedy, including change to the seaside. In this case, nothing seemed to check the morbid process, or to set up healthy action, till iodoform was called into requisition. Under its use, the ulceration had almost healed, when the patient was lost sight of. I have often injected the deep sinuses that may result from buboes, &c., with a solution of iodoform, and have frequently found them mend under this treatment when other means have failed. As an injection in gonorrhœa, in the few cases in which it was tried, it seemed to set up so much inflammation, that I abandoned its use.

Chronic Ulcers.—In ulcers about the lower extremities, and indeed elsewhere, I have formed a very high opinion of iodoform as a therapeutic agent. I have used it largely both at the hospital and in private. Ulcers that have remained open for years, and on the treatment of which much care and skill have been expended, often close in a few weeks under its influence; but the same caution must be repeated as in the case of venereal sores. It will only irritate the actively inflamed wound. It is the indolent ulcer, from whatever cause it may arise, whether from varicose veins, malnutrition, syphilis, or injury, that is especially benefited by iodoform. Repeatedly, under its use, I have seen a surface, glazed or œdematous, rapidly take on healthy action, granulate, and heal, and this where other measures have been tried for months, or even longer, without effect. Often, too, the pain that so frequently accompanies these ulcerative processes ceases after iodoform has been applied for a few hours.

As a Parasiticide.—In many cases of ringworm of the scalp of long duration, and which had been before the subjects of much and careful treatment, I have prescribed iodoform in the form of an ointment. In several of these, speedy improvement ensued, spores being no longer to be found and the parts returning to a state of health; but I met, in some instances, with considerable difficulty in inducing the parents to apply the remedy, on account of its powerful odour. It set up no violent

inflammation, and I hope it may prove an useful adjunct to the means at our disposal for combating that disease.

Chloasma quickly yields to this agent; but, again, its odour is an insuperable objection to its employment in the treatment of this disease. The results that I obtained from its application in several cases of sycosis were not encouraging, as it seemed to give rise to undue irritation. In the form of powder, I have used iodoform in several cases of lupus with ulceration and rodent ulcer; but my observations on its conduct in these cases have not been at present sufficiently complete to warrant a definite opinion.

A word, in conclusion, as to its mode of application. If used as a powder, iodoform should be dusted on the ulcerated surface, and a piece of dry lint, or lint soaked in a weak solution of carbolic acid, may be laid over it, and this process repeated night and morning. Undiluted, I have often found it apt to produce irritation and pain, and, therefore, generally prescribe it mixed with equal parts of either fullers' earth or tannin. As a parasiticide, I have used it as an ointment with about twenty grains to an ounce of lard, and have directed it to be applied twice daily. Such an ointment spread on lint is a convenient mode of application to a wound or ulcer, and its employment in this form prevents the risk of dropping this disagreeably smelling drug on the patient's clothes, &c. If an ointment of the strength named cause inflammation or pain, it may be diluted. I am also in the habit of ordering iodoform in combination with a salt of mercury, &c., with satisfactory results. So also it may most conveniently and easily be applied by painting the part with its solution in alcohol, chloroform, or ether.

There are two drawbacks to the use of iodoform. The first is its extremely disagreeable odour, which, unless it is carefully covered over, scents the room in which the patient is; the second is its high price.—*British Medical Journal, Feb.* 9, *p.* 190.

91.—THE THERAPEUTIC PROPERTIES OF SALICYLIC ACID.

H. Köhler gives as the result of a large number of experiments with salicylic acid and its salts—1. That in febrile conditions both salicylic acid and sodium salicylate lower the temperature. 2. That in large doses salicylic acid depresses the respiratory activity, and may even cause death by asphyxia. 3. Salicylic acid does not exist in the blood in the uncombined state, but as sodium salicylate, in which state it is eliminated in man at least by the kidneys. 4. Salicylic acid acts as an antiseptic but sodium salicylate arrests neither fermentation nor putrefaction. In this point of view it is remarkable so long as we admit the identity of the action of antiseptics and of the antifebrile action of certain drugs that both salicylic acid and sodium salicylate lower the temperature in typhus, articular

rheumatism, and the febrile state of phthisical patients. Köhler set himself the task of determining whether salicylic acid alone or both it and its salts possessed the power of lowering the temperature in healthy dogs, and he was led in the first instance to notice its effects (*a*) upon the respiration, and (*b*) upon the circulation. He found that both in dogs and in rabbits salicylic acid and sodium salicylate when ingested into the stomach or injected into the blood, rendered the respiration slower, apparently by depressing the excitability of the sensory nerves, *i.e.* the branches of the vagus in the lung, leading to asphyxia by carbonic acid poisoning. In regard to the circulation he found that when quickly injected and in large quantities the blood pressure rapidly sunk almost to zero, followed by convulsions and death, or in some instances, by great retardation of the cardiac movements with sphygmographic curves of immense height, and a more gradually occurring fatal issue. The causes of this depression lay in the heart itself, and appeared to be due to the action of the drug on the ganglionic apparatus or to paralysis of the muscular tissue of the heart. In regard to sodium salicylate, though not acting so rapidly or powerfully as salicylic acid, its effects are essentially the same. The cardiac branches of the pneumogastric have their excitability lowered and before death are entirely paralysed, and the same results occur from the ingestion of sodium salicylate into the stomach as from the injection of salicylic acid into the blood. In regard to the distribution of temperature he found that in healthy rabbits, cats, and dogs, the injection of from six to ten cub. cent. of salicylic acid or sodium salicylate into the jugular vein or the ingestion of from ten to twenty cub. cent. of solution of sodium salicylate into the stomach caused a depression of temperature of about three degrees Centigrade (or 5° Fahr.) just as occurs in man during the febrile state. But inasmuch as the sodium salicylate does not possess any antagonistic properties to fermentation and putrefaction, whilst salicylic acid is immediately converted into sodium salicylate after absorption, there is strong reason for not regarding antiseptically acting substances as being also antipyretic. The diminution of temperature observed after their administration can only be partially explained by the above-mentioned modifications of the functions of respiration and circulation, though it undoubtedly goes hand in hand with them; possibly it may be due to some influence on the vaso-motor nerves leading to dilatation of the peripheric vessels, and consequent lowering of the temperature of the blood; this hypothesis affords an explanation of the congestions of the head, singing in the ear, profuse perspirations, and diuresis observed in many animals.

From all this the following conclusions may be drawn:—

1. That it is only when externally or topically applied that salicylic acid acts antiseptically. 2. When salicylate acid is ingested by the mouth it loses its antiseptic action because it becomes united with an alkali, and it then only acts on the economy like sodium salicylate when taken internally. 3. Both salicylic acid when taken internally and salicylate of soda, though destitute of any antiseptic action, are yet capable of depressing the temperature of the body in febrile conditions to an extent unapproached by any other remedy.

As rules for practice Köhler considers that salicylic acid is exclusively to be used as a topical antiseptic, and in this point of view it is extremely valuable in diphtheritis of the tonsils, pharynx, and nose, where it may be applied in the form of solution containing one part in three hundred. Also as a prophylactic remedy in infectious blenorrhœas, in leucorrhœa. and chronic cystitis, and in dysentery and diarrhœa ; and it is also useful in dyspepsia and the migraine accompanying it, preventing fermentative processes in the stomach; for this purpose the ferro-salicylate is best adapted. Sodium salicylate can be more readily used on account of its greater solubility. One drachm and a half may be given once or twice in twenty-four hours, which acts more promptly in reducing the temperature in fever than quinine and cold water. Its lowering action on the temperature very rarely fails in acute articular rheumatism, diphtheritis, typhus, or local inflammations. Singing in the ears and deafness are the only inconveniences that occur from the use of sodium salicylate.—*Practitioner, May* 1878, *p.*376.

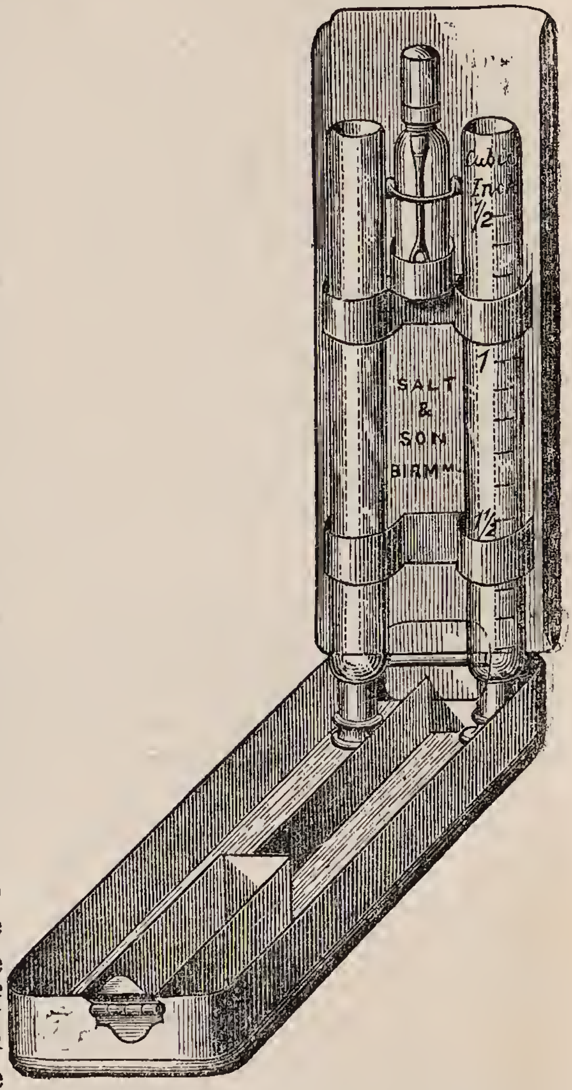
92.—DUBOISIA MYOPOROIDES.

This plant, of which an interesting account was given by Dr. Ringer and Mr. Tweedy in the Lancet, March 2nd, is probably destined to become an important addition to our therapeutic agents. As it closely resembles atropia in its action, the following points of difference between the two alkaloids, as given by Mr. Gerrard, F.C.S., in a paper just published, are worthy of note. The solubility of duboisia in water is twice or more than twice that of atropine. It has more power in neutralising acids than atropine. Its behaviour to sulphuric acid (cold), and also when heated with bichromate of potash, differs from that of atropine. When boiled with baryta, the odour it evolves is entirely different from that given off by atropine under the same conditions. It will be seen from Dr. Ringer's elaborate report that "the physiological action of the extract of duboisia is apparently identical with that of atropia. The same remark applies to the effects of the local application to the eye. If there be any difference, it is that duboisia is more prompt and energetic than atropia, and certainly very much more so than the strongest extract of belladonna."—*Lancet, April* 20, *p.* 593.

93.—NEWLY INVENTED APPARATUS FOR TESTING THE PRESENCE OF SUGAR IN URINE.

By Dr. Thomas Birt, Leamington.

This little apparatus, of which we give an engraving, drawn to a scale of $\frac{1}{4}$ size, has been arranged by Messrs. Salt & Son, Birmingham, at the suggestion of Dr. Birt, of Leamington. The case consists of two stoppered bottles, one plain, and the other graduated to a scale of tenths of cubic inches, both having glass stoppers perforated by a capillary opening, with cross-shaped notches on their outer surface to allow the escape of fluid. Both bottles are held in position by slight metallic clips, so that the lid may be closed. There is also a small bottle for yeast (which is the ferment employed), stoppered and capped, and provided with a small spoon which serves as the measure for the ferment used. The trough seen at the bottom of the case becomes a a kind of pneumatic trough, being just covered with a stratum of the urine to be subjected to experiment. The bottles are to be taken out of the clips, filled *quite full* with the urine, firmly stoppered, the graduated one having previously been treated to the modicum of yeast, when both are to be inverted and pressed closely on the bottom of the case, as shewn by the engraving; any superfluous urine escapes through the opening of the stoppers, leaving the bottles full and free from air. The case is then to be exposed to a gentle temperature, when, if sugar be present, fermentation soon begins, gas is eliminated, collecting in the upper part of the bottle, and expelling a portion of the urine through the opening in the stopper. The second, or non-graduated bottle, having received no ferment remains unaffected, and lying side by side with the other offers an easy and ready comparison.—*Vide British Med. Journal, March* 2, 1878, *p.* 293.



94.—AN EFFECTUAL ANTIDOTE TO BAD AIR.

By Dr. R. H. Goolden, London.

[The following communication appeared in a letter addressed to the editors of the Leeds Mercury, by Dr. Goolden.]

A leading article in the Times and some subsequent letters on the ventilation of law courts has led me to believe that the suggestion of a very simple remedy may be useful, at all events until time is given to carry out a good system of ventilation; and as the cost need not exceed one shilling, and the trouble is very little, perhaps the Corporation of Leeds would do me the favour just to try the remedy I suggest. It has been tried on small and on very large scales, and never failed in its results. It is used in hospitals, large schoolrooms, slaughter-houses, tanners' yards, the lower decks of ships, where several hundred seamen have made the atmosphere so impure during the night as to be most offensive to any one from the outside suddenly entering. It is reasonable, and based upon the laws of physical science, and so simple that one of our best practical chemists said, "I wonder that it never occurred to us before." It is just the application of a very weak solution of chloride of lead, suspended in the atmosphere on a towel in the room, and in considerably less than one minute the air is deprived of the slightest trace of anything offensive or deleterious. The room may be of any dimensions, and so long as there is any lead in the solution there cannot exist any of the poisonous gas in the atmosphere; in fact the confined air of a hospital becomes much purer than that of any large town, as may be proved by its failing to tarnish the most delicate silver work or silver lace after months of exposure.

I will not occupy your valuable space by any scientific explanations, as they are to be found in the Lancet and in the Times, further than necessary for the application of this result of many careful experiments.

I would only wish first to disabuse the public of a notion—that the evolution of carbonic acid gas as the noxious element is the cause of the mischief we want to rectify. The offensive matter is sulphide of ammonium, hydro-sulphuric acid, and other still more poisonous gases of which sulphur is the base. So poisonous is this gas that one volume added to 1,500 volumes will kill a small bird: and one volume to 1,000 will kill a middle-sized dog instantly; and one volume added to 250 its volume of air will kill a horse. This when existing in the atmosphere in an exceedingly small quantity is most injurious to animal and vegetable life—in fact, it is poisonous in proportion to its quantity. It is constantly secreted from the surface of the living body, and evaporates into the air. When crowds

of people are assembled in a room, the air becomes very poisonous; but fortunately the chloride of lead destroys it, by attracting it with the strong force of double electric affinity. The sulphur combines with the lead and hydrogen with the chlorine, and the sulphur will combine with nothing else if lead be present, and the combination is perfectly insoluble, so that it involves no fear of lead poisoning.

The mode of using it is :—Take a drachm of nitrate of lead, and dissolve it in a pailful of soft water (rain water or distilled,) and take a drachm of common salt, which dissolve in a jug of soft water, and when the solutions are mixed it is ready for use. Dip a towel into it and hang it up in the offensive room. The strength might be very much increased, but for ordinary purposes I find this quantity quite sufficient; and if not, more may be made with very little trouble. It is important to understand why soft water should be used. If there is any lime in the water there will be carbonic acid, and a precipitate will be formed of carbonates of lime and of lead. If it is unavoidable, the precipitate must be allowed to fall, and prevented from mixing with water or sewage falling into rivers or springs, otherwise you may get lead posoning.

I do not wish to interfere with any scheme for ventilation any person or public body may contemplate; but this may be used in conjunction with it as it may be applicable, and would be very much better than the plan of forcing the air through filtering gauzes, as described in one of the letters to the Times.

I shall be happy to give any further information either to your Leeds engineers or sanitary officers if they reqnire it, and only ask in return a statement of results, which may be useful for scientific and not for commercial purposes. It must be observed that it will not affect the smell of coal gas, but will entirely separate all sulphuretted hydrogen from coal gas, and add greatly to its illuminating property. Nor do I propose it as a disinfectant, though it may be such under certain conditions, as in the case of the Thunderer at Haslar, where all the cases of typhus were restored when my solution was tried as I directed. My object there was to purify the air of a sick room and my hospital wards, which I succeeded in doing without ventilating my poor patients to death; and I hope, if I live a few years longer, to find that no cottager or artizan in the country may be without the means of living in a pure atmosphere for less than a farthing's cost. It will assuredly lessen the craving for beer and alcoholic drink, and increase the working powers and health of the people.—*Leeds Mercury, March* 23, 1878.

95.—AIR FILTERING.

At the last meeting of the Manchester Literary and Philosophical Society, Dr. R. Angus Smith in the chair, Mr. William Thompson, F.R.S.E., read a paper "On the construction of a room, or series of rooms, free from germ life proposed for use in the performance of surgical operations." The paper was illustrated by a model room and apparatus. Mr. Thompson said his object in bringing the paper before the Society was to show what he considered to be a valuable application of a well-known principle—namely, that of filtering from the ordinary air the innumerable spores that are constantly found floating about in it, and so to arrange a room or series of rooms in which the air may be rendered optically pure. He described a series of experiments by which he had succeeded in excluding fungus spores from a confined space or model room—the process being that of filtration through layers of cotton wool—and said that the results obtained showed conclusively that ordinary large rooms might be constructed and ventilated with filtered air by means of fans, so that flour paste, taken as a test standard, would remain in them free from fungus life. Such a room, or series of rooms, might be of great advantage in surgery, perhaps as a means of preventing spores from entering the wounds of patients, and so doing away with or lessening the onus put upon the antiseptic treatment, or in giving a better chance of success in serious surgical operations. The arrangements which he suggested on a large scale were a long room, or series of rooms, in a line at one end of which should be fitted a fan, behind a good filter of cotton wool, a long pipe, with a series of Bunsen burners set along it, so that when all were lit a line or sheet of flame would be produced, which might be gently passed along and made to play on every part of the floor, walls, and roof of the room, beginning near the end at which the fan worked and going gradually towards the door. By this means any spores adhering to the walls would be destroyed, and no air could pass back to pollute the walls or floor which had been thus purified. A stove might be arranged at the door end of the room, by which cotton garments to cover the ordinary clothes of the surgeons and attendants might be heated to a temperature presumably sufficient to destroy or paralyse the vitality of any spores that might have been adhering to them, where the knives and other appliances used might be previously heated, and where water used in washing wounds might be previously heated under pressure. With such an arrangement at an hospital, it seemed to him one interesting mode of investigation into some most important subjects might be commenced.—*Manchester Guardian, Friday, Dec.* 7, 1877.

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TO

BRAITHWAITE'S

RETROSPECT OF MEDICINE,

1875—1877.

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PRINTED FOR D. I. ROEBUCK,
AT THE OFFICE OF THE YORKSHIRE POST, ALBION STREET, LEEDS.

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